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PREFACE

The objective of the *Annual Review of Psychology* is to provide expert evaluations of progress both in the main traditional areas of psychology and in important new or developing areas. In order to assure such coverage, the Editorial Committee has devised a master plan of topics, and the plan is revised frequently. The master plan of 1970 is shown below. We present it here to give information to our readers and to solicit comments and suggestions.

As the table shows, certain topics are included in each volume (those indicated by "1"), certain topics occur every 2, 3, or 5 years, and some appear only infrequently. For a number of reasons, no volume follows the plan exactly. Sometimes a chapter will reflect a more restricted coverage than that of the topic on the master plan, since authors are encouraged to be selective. In almost every volume, some planned chapters do not appear because authors defer their contribution or default. Nevertheless, the plan states our aim for coverage, and we adhere to it reasonably well. Comments about the plan and suggestions for its revision will be welcomed.

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* The numbers represent the intervals in years between successive chapters. The Xs indicate only occasional appearance.

* * * * *

In 1970 George W. Albee completed his five-year term of membership on the Editorial Committee. Dr. Albee typified the faithful service that has characterized members of the Committee, since he met with us regularly during years that were filled with many other responsibilities, including the presidency of the American Psychological Association. We are happy to welcome Dr. Leona Tyler as the incoming member of the Committee. It should be noted that the topics and authors for the present volume were chosen by the Editorial Committee for 1968, whose names appear opposite the title page. Miss Jean Heavener, the assistant editor, has been our cheerful and efficient collaborator in all phases of organizing and producing this volume. We also wish to thank Mrs. Dorothy Read, who has again worked rapidly to prepare the accurate and detailed subject index.

M. R. R.
P. H. M.

ERRATA

Volume 21

P. 346, line 34: *for 50 msec read 500 msec.*

P. 352, sentence beginning on line 35 *should read:*

This result is also consistent with the single-band model: when signals are present in both channels, the observer is always attending to a channel with a signal present; when a signal is present in only one channel, the observer will sometimes be attending to the wrong channel.

REPRINTS

The conspicuous number (160 to 174) aligned in the margin with the title of each review in this volume is a key for use in the ordering of reprints.

Reprints of most articles published in the *Annual Reviews of Psychology* and *Biochemistry* from 1961 to 1970 and the *Annual Reviews of Microbiology* and *Physiology* from 1968 to 1970 are now maintained in inventory. Beginning with July 1970, this reprint policy was extended to all other *Annual Review* volumes.

Available reprints are priced at the uniform rate of \$1 each postpaid. Payment must accompany orders less than \$10. The following discounts will be given for large orders: \$5-9, 10%; \$10-24, 20%; \$25 and over, 30%. All remittances are to be made payable to Annual Reviews Inc. in U. S. dollars. California orders are subject to sales tax. One-day service is given on items in stock.

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The sale of reprints of articles published in the *Reviews* has been expanded in the belief that reprints as individual copies, as sets covering stated topics, and in quantity for classroom use will have a special appeal to students and teachers.

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- DEVELOPMENTAL, *Lewis P. Lipsitt and Peter D. Eimas*
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BASIC DRIVES

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FRANK W. FINGER AND DOUGLAS G. MOOK

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"We recommend that future... chapters... be devoted to content areas such as 'hunger and thirst,' 'sex and maternal behavior,' and the like." The Editorial Board either failed to read or decided not to heed this next-to-last sentence in the initial effort, two years ago, to "cover" the "basic drives" for the *Annual Review of Psychology* (43). Since the Board outnumbered us, even when we were reinforced by the previous collaborative team, we meekly accepted our literal assignment and disappeared into the library. Our first action on emerging some weeks later was to weigh our accumulated bibliography cards and, by a simple calculation, ascertain that the pages reserved for us would barely accommodate all our references through 1969, to say nothing of any textual material. Something obviously had to give. That our manuscript now appears indicates that the Editors were willing to go along with our unilaterally determined solution: (a) We will not add yet another discussion of what, if anything, drive is. (b) We will restrict our attention principally to just a couple of topics that interest us, without trying further to justify our choice. (c) When "Literature Cited" threatens to exceed 20% of our allotted space, we will begin to cull, cheerfully and not entirely whimsically. We will favor the more recent articles on the assumption that they in turn will reference earlier ones. (d) We will limit our apology and catharsis to this very brief Introduction.

SEXUAL BEHAVIOR

There has been very little coverage of research on sexual behavior in the first 21 volumes of the *Annual Review of Psychology*, in part because of the availability of summaries in other form (e.g. 10, 11, 63, 219). In introducing some of the recent experimental work, we have been forced by stricture of space to depend on these sources, together with a current review chapter by Davidson (57), for providing background discussion and more complete bibliography. We have focussed primarily upon copulation itself, from which it follows (cf. 10, pp. 535-37) that the investigations cited involve for the most part rodents and carnivores.

Description of sexual behavior.—Several detailed descriptions of the dog's mating have recently appeared (e.g. 14, 102). Hart (102) provides quantitative data not only for the various components of the intact male's

behavior, but also for the reflexes elicitable in the spinal preparation (shallow thrusting, lordosis, the "intense ejaculatory response," and the simulated lock), with electromyographic recording supplementing photography. Beach (15) details the behavior of both partners after premature termination (more than 10 sec but less than about 5 min) of the genital lock. The male will sometimes exhibit a "phantom lock" of several minutes' duration: he stands nearly motionless in the posture characteristic of the normal lock, with erection maintained and rhythmic ejaculatory contractions continuing, unusually unresponsive to environmental stimuli. The female meanwhile is very hyperactive, barking, mounting the male repeatedly, and licking the penis (which hastens its detumescence and the possibility of resumption of mating). Beach's neurophysiological speculations based on these observations extend his 1967 discussion (11).

Dewsbury's (62) moment-by-moment analysis of the rat's behavior during mating sessions involves such categories as locomotor-exploratory, grooming, sniffing partner, naso-nasal contact, and immobility, as well as the more obviously sexual responses. Particularly striking is the temporal distribution of immobility, which peaks (more clearly in the female) just after ejaculation. He hypothesizes that this pattern reflects seizure-like neural activity. Folman & Drori (81) describe the changes in the male pattern (as well as in the accessory organs and hypophysis) as a function of number of ejaculations permitted over a 4-week period.

The sexual behavior of the gerbil follows the same general pattern as the rat's, with multiple bursts of intromission before ejaculation is achieved, a number of ejaculations in a session, a decrease during the session in number of intromissions required for ejaculation, and an increasingly prolonged refractory period following the successive ejaculations (130). There are, however, significant quantitative differences, with the gerbil slower to mount and the number of his intromissions per ejaculation consistently the greater. Foot thumping (principally in the male) decreases as sexual arousal declines during the session, and is interpreted to be an integral part of the courtship ritual.

The chinchilla is more similar in mating pattern to the guinea pig (like it, a member of the rodent suborder *Hystriagnomorpha*) than to the rat (28). The female is inconsistent in her willingness to stand in copulation, and frequently interrupts intromission by moving away from the male. Ejaculation occurs during the first insertion, after just a few seconds of thrusting. Thereafter the female's receptivity is terminated for several hours, and perhaps for the rest of the (relatively infrequent) estrus.

When inter- or intraspecies comparisons of sex behavior are made, precise specification of descriptive measures is required if ambiguity is to be minimized. It may be desirable, for instance, not just to record the occurrence of lordosis in the mounted female, but to apply an ordinal scale of intensity or quality of lordosis (105). Further, while different indices of sexual state may covary under certain conditions, it is a serious error to assume that cor-

responsiveness is the unexceptionable rule. Cornified vaginal smears, greatly elevated general activity, strong lordosis, and minimal resistance to mounting typically occur in close temporal proximity during the female rat's estrous cycle, but disjunction of these signs has been reported in a number of circumstances. Neonatal deoxycorticosterone treatment produces an adult with greatly deficient lordosis in spite of a normal smear cycle (204), adult females which have been exposed to androgen prenatally fail to mate in spite of the development of general activity and estrous smear cycles (120), discrepant thresholds to estrogen treatment are reported for estrous smears and receptivity (58), receptivity can persist after abolition of normal vaginal cycles by environmental stress (91), some individuals show continuous estrus in terms of smears and lordotic response to fingering but vary greatly in receptivity to the male (2), and the female may kick at the attentive male but display lordosis when his persistence finally results in mounting (100). A neurophysiological model with just one mediating mechanism for sexual behavior would obviously be too simple.

Neurophysiological correlates.—Many experiments over the past decade have manipulated rat hormone level in early life by gonadectomy and/or hormone administration (see 10, Ch. 5; 57; 63, Ch. 5). For example, genetic females injected (105, 155) or topically treated (73) with testosterone propionate (TP) in infancy, will in adulthood fail to display normal female behavior, even with estrogen (estradiol benzoate) and progesterone exogenously supplied, and under the influence of androgen may mount more frequently than Ss lacking the early treatment. On the other hand, castration at birth permanently reduces the male rat's ability to ejaculate and increases his tendency to give feminine responses, when tested under the appropriate hormone; protection against this alteration is inversely related to the age at which TP supplement is given during days 1–10 (19). This sort of finding has most often been interpreted to mean that the neural substrates for both masculine and feminine behavior are originally available in both male and female rodents, and that the presence of androgen during a critical early stage suppresses the development of the substrate of feminine behavior. Whether the androgen normally present in appreciable amount during the first few postnatal days (166) also organizes the masculine neural mechanisms is subject to dispute (215). A complicating issue is that one effect of early androgen is to sensitize the external genitalia so that there is rapid growth in response to TP in adulthood (19, 105); the consequent increased sensory feedback may be at least part of the reason for the more complete male sex pattern in Ss having received androgen, endogenous or exogenous, in prenatal or perinatal life.

That there are two separable effects of early androgen is suggested by experiments substituting the less potent androstenedione for testosterone. Goldfoot, Feder & Goy (88), by injecting neonatally-castrated male rats with androstenedione during the first 20 days of life, produced individuals

with uniquely bisexual behavior: under TP in adulthood they displayed normal male intromission responses, but under estrogen and progesterone gave lordotic reactions resembling females'. Stern (191), finding a similar paradoxical condition in one of his groups, refers to them as being simultaneously masculinized and not masculinized (or not defeminized). If indeed there are two types or processes of masculinization (defeminization), it does not necessarily follow that both involve central mechanisms. Penis size after TP supplement in adulthood was significantly greater in the early-injected than in the noninjected Ss, even with this weaker androgen (88), and it may have been the enhanced peripheral stimulation from this organ that supported masculine copulation in individuals that still could display feminine lordosis because of lack of neural masculinization.

The possibility of at least partially independent neural centers for male and female behavior finds support in Singer's (180) extirpation study in the female rat. Lesions in the anterior hypothalamus completely eliminated lordosis (under injected estrogen and progesterone), while interfering only moderately with mounting (under TP). In contrast, preoptic lesions severely impaired masculine responses but had no effect on lordosis. If these observations and those earlier described by Loit (141) are confirmed, it may be that the control of so-called male behavior differs in males and females, for the latter investigator found no reduction in copulation by males after damage to either medial or lateral preoptic areas. Rodgers (167) reports that the "basal hypothalamus is necessary for the expression of copulatory behavior as well as gonadotropin secretion in male rats" (p. 465), and Rodgers & Law (168) find that habenular lesions significantly depress lordosis quotients in the female.

Sympathectomy is reported to abolish ejaculation in male guinea pigs, rabbits, rats, and cats—but it does not in dogs (13). While the operated mount less frequently, they nevertheless are capable of effectively organized sexual behavior culminating in the genital lock. Anal contractions usually indicative of ejaculation are observable, and the duration of the lock does not differ from that in a normal group.

Moderate activation of adrenergic pathways (low doses of LSD-25, *d*-amphetamine, methamphetamine, cocaine) tends to facilitate sexual behavior in male rats, in terms of decreased latencies (27, 137). Disruption of mating follows administration of such anticholinergic agents as diazepam, scopolamine hydrobromide, benactyzine hydrochloride, and atropine. In females, atropine virtually abolishes both lordosis (under estradiol benzoate and progesterone) and mounting (2 weeks of TP), without impairing general movement (179).

On the basis of earlier evidence that serotonergic mechanisms inhibit the lordotic response in estrous females, Tagiamonte et al (199) reduced the brain-serotonin concentration in male rats to less than 30% of normal by administration of *p*-chlorophenylalanine and pargyline. A high level of sexual excitement, including mounting and the intromission pattern, was directed

toward their male cage mates, and was promptly eliminated by injection of serotonin precursor. It is concluded that "Brain serotonin inhibits sexual behavior in male rats," and that "the relative balance of serotonergic and noradrenergic tone in brain may control sexual behavior in male animals" (p. 1335).

Individual and age differences.—Individual differences among intact males in ease of arousal and amount of consummatory behavior seem most readily accounted for in terms of differential sensitivity of neural mechanisms to androgen, rather than immediate levels of circulating hormone. Thus Larsson (133) castrated sexually active and sexually sluggish rats and later gave both groups large replacement doses of TP; as recovery took place the original group separation was maintained, in spite of current hormonal equivalence. Similarly, precastration individual differences in hamsters' spontaneous emissions were duplicated under equal androgen therapy (17).

Is the final level of sensitivity of the neural mechanisms to hormones reached by the end of the critical perinatal period? Larsson (134) argues otherwise. He demonstrated first that the 200-day-old rat's ability to achieve ejaculation after fewer intromissions than required by the pubertal male is due to neither experience nor nongonadal correlates of age. In his major experiment he castrated Ss at 30 or 160 days (i.e. after the testes had provided androgen for differing durations) and 4 months later initiated sex tests with exogenous TP supplied. On the first test the usual pubertal-adult difference was observed, the early castrates requiring nearly twice the number of intromissions to ejaculation as did the group castrated in adulthood. The effect of a given amount of androgen therapy depends, in other words, upon the long-range hormonal history of the individual. Since performance differences disappeared by the second mating session 6 days later, and since it is doubtful that daily injections could so quickly produce significant penile growth in the early castrates (85), it is tempting to identify central mechanisms rather than the peripheral effector-receptor instrument as the site of the effect. It seems reasonable to conclude that the role of androgen after the perinatal period is not limited to the immediate priming of the mating response, but that hormonal organizing effects continue into adulthood. Beach & Westbrook's (22) demonstration that the genital organs maintain their normal condition but mounting is severely impaired after replacement of endogenous androgen by the synthetic androgen fluoxymesterone, suggests a technique for testing the reversibility of this organizational process, with a few days' potentiation of the sex behavior by TP (cf. also 88).

The role of experience.—It would be surprising if so complex a pattern as courtship and copulation were independent of the effect of experience in a class as highly evolved as Mammalia. It is reassuring, therefore, to have evidence from several directions that restriction of social contact early in life reduces the mating efficiency of males, and that the rat does not after

all violate the principle already established in species as diverse as guinea pig and rhesus monkey (e.g. 147).

Beach (12) removed male beagle puppies from the litters 3 weeks after birth and for more than a year kept them in individual cages except for a very few brief exposures to their fellows. In contrast to these semi-isolates, control Ss were allowed daily 15-min periods of group interaction. When tested at 13-14 months with females artificially brought into estrus, the semi-isolates were clearly deficient. The principal problem seemed to be their faulty postural orientation toward the female, with side mounts as frequent as rear mounts. Two never achieved intromission, even though given ten daily opportunities lasting 10-30 min. The other three experimental animals effected insertion on only 1/5 of their mount-positive tests, compared to an average of 69% for the controls. There seemed to be no difference between the groups in interest or motivation, for the mounting scores (rear plus incorrect) were essentially equivalent. Nor did the treatment adversely affect such reflex components as thrusting and lock maintenance. On retests 6-19 months later three of the semi-isolates were still distinctly inferior in their behavior. No attempt at remedial action is reported.

It should be pointed out that the record of the control dogs, permitted only the short daily encounters with conspecifics, was comparable to that of males living from weaning until testing in a 10-member heterosexual pack (with the females removed during estrus). More precise definition of the threshold for impairment will require detailed manipulation of the several experiential parameters.

Does the behavior of the socially reared male dog change as he is permitted repeated sexual opportunity? Hart (102) describes a number of trends, such as reduction of delay in dismounting after completion of the lock. The mean duration of the intense ejaculatory response decreases significantly ($p < .05$) from .40 min in matings 1-7 to .35 min in matings 19-26, the duration of the lock from 18 min to 15 min (the test female in induced estrus). Beach's (12, Exp. II) failure to find reliable changes over trials could be due to his smaller number of Ss and the greater variability that may characterize tests involving bitches in natural heat.

The results of three experiments agree that rats living in isolation from weaning until maturity are inferior copulators, with the more severe disability following the earlier separation. In an investigation by Gerall, Ward & Gerall (86) only 1 of 30 males housed individually from 14 days achieved intromission on the first test, compared to 14 of the 27 maintained three to a cage; 23% and 100%, respectively, eventually mated at least once (nine tests). Physical contact seems to have been the critical variable, for completely isolated Ss were not appreciably inferior to those which were simply separated from others by wire mesh. The nonmating rats could be described as responsive but unskillful—they promptly investigated the stimulus female, but either interrupted the preliminary courtship sequence by tunneling under or clambering over her, or mounted her clumsily. Very similar

aberrant patterns were observed in Folman & Drori's (80) segregated animals, all of which exhibited persistent interest in the female but failed without exception to execute a properly oriented mount. The courtship pattern of males whose isolation begins on day 1 is even more rudimentary (90), although the issue may be clouded by the subnormal rate of growth of the artificially nursed pups.

It is unclear what sort of communal living is required for maximal mating proficiency. Gruendel & Arnold (90) believe that infantile contact with a mature female is a requirement for which peer contact is an inadequate substitute; unfortunately, their experimental design lacked a motherless group given peer experience until adult testing. The other side of the coin is described by Hård & Larsson (97), whose data indicate the need for the presence of age mates during infancy and thereafter. Gerall, Ward & Gerall (86) found that their homosexually reared rats performed as well as those whose trio included a female, but the copulation scores of Folman & Drori's (80) socially experienced males were higher when the group included hysterectomized females. To complicate the matter, Gerall, Ward & Gerall (86) report that "therapy" of three noncopulating isolates, in the form of living for 2 weeks with a normal female, was successful, but that treatment providing a spayed female was a complete failure. Finally (157), after homosexual maintenance from weaning to 4 months, males were housed for 12 days either with continuous access to estrous females or individually within sight and smell of estrous females and other males. In the immediately subsequent test the heterosexually experienced rats not only achieved a greater number of intromissions than their naive confreres, but also seemed to have higher drive as reflected in more frequent pursuit and mounting. Any attempt to resolve the seeming contradictions must take cognizance of the retardation of reproductive-system development in males reared for 15 weeks without females—approximately equal under conditions of isolation and homosexual grouping (205), and the apparent elevation of androgen production and consequent hypertrophy of the accessory organs with increasing opportunity for ejaculation (81).

Critical sense modalities.—We have already intimated, and will document in more detail below, that successful copulation can be prevented by reduction of genital stimulation. What is the sensory basis of the male's initial interest in the estrous female? In the rat, according to Beach (63, p. 119), arousal depends "upon a multisensory pattern in which several modalities are involved and no one is essential." There have been numerous observations, however, that indicate differential importance of the modalities, with olfaction high on the list and vision low. Recent data underline at least the second half of this generalization. Neither variation in illumination level nor enucleation significantly affected the experienced male's mating score, although in the latter circumstance he occasionally had difficulty locating the female promptly within the testing arena (99).

In response to Heimer & Larsson's (104) "conclusive evidence of the important of olfactory stimuli in the male rat mating behavior" (p. 209), Bermant & Taylor (25) examined the interaction of olfactory bulb damage, experience, and change of coital partner. While the duration of the ejaculation series was prolonged after ablation, they failed to confirm Heimer & Larsson's report of lengthened intromission latency and decreased number of ejaculations. They found it difficult to account for the complex pattern of changes "by reference to the males' loss of ability to smell the females" (p. 17). Larsson (135) has examined the problem in further detail, verifying by an odor preference test that the bulbectomized rats were indeed anosmic. Prior to surgery all Ss had ejaculated on each of three 30-min tests, and the sham-operated controls continued to perform efficiently. The anosmic animals, in contrast, were markedly deficient. Mounting occurred in only 60% of their tests and intromission in 53%; ejaculation was abolished in 41% of the animals, with an overall incidence of only 20%. Both intromission latency and length of ejaculation sequence were increased significantly. Injection of highly potent testosterone or a gonadotrophin had no effect; this finding and the absence of histological defect in the operated testes argue against interference with the pituitary-gonadal system as a tenable explanation for the behavioral loss. The lowered excitability, Larsson hypothesizes, results from damage to a neural mechanism involved in the integration of reproductive patterns. The substantial role of the olfactory system in sustaining mating is understandable in view of its considerable afferent input to the medial preoptic-anterior hypothalamic continuum, itself crucial in male behavior (103; but cf. 141).

There is evidence that olfactory stimulation is not only facilitating but in the male golden hamster an absolute requisite for motivating male sexual behavior (156). After bilateral bulbectomy, no S (experienced or naive) made any mating response. Since not even castration suppresses mounting so completely, and since TP injection had no restorative effect, it can be presumed that hormonal deficit was not implicated. Actually the behavior change went beyond the narrowly sexual: the failure to evince the slightest interest in the female was but one aspect of a general reduction in interindividual behavior. Thus normal territoriality, characterized by scent-marking the cage and attacking intruders was lacking. It may well be that the extension of such studies to additional species will not only force a revision of the doctrine of "multisensory dependence" of sex behavior, but at the same time make it more abundantly clear that "sex" is most fruitfully examined in the context of a broad range of social patterns.

Variety.—It has been demonstrated in several species that the satiated male will resume mating if presented with a new partner (for references on this "Coolidge effect" see 20). Further analysis of the phenomenon in the rat has indicated that if the original mate is simply removed for a moment and then put back into the testing arena the male will often resume copulat-

ing with her, although not reaching as great a total number of ejaculations as if a different female, and especially an unmated female, is supplied (for a failure to verify, see 113).

When preference is substituted for amount of consummatory behavior as the measure of the effect, a somewhat altered picture emerges (220). After the male has achieved three ejaculations with the original partner, she is removed from him for 15 sec and then she and a fresh estrous female are simultaneously presented to him in the test chamber. In this case the male does not favor either female. However, if the just-mated partner is returned to the choice situation after having received six ejaculations (the first three with a "warm-up" male and the last three with the experimental male), she is significantly less attractive than the new partner. This does not reflect a simple aversion for mated females, with the threshold between three and six ejaculations. If all the previous servicing has been provided by warm-up males, the six-ejaculation female is selected at least as often as the unmated female, and this holds true for semi-satiated as well as rested males. In short, the female's incentive value is in part a function of her novelty as a partner. The discrimination may well depend on olfactory cues (127).

Does the arousing value of change extend to the physical setting in which the sex tests are given? While reports indicate that in dairy bulls and water buffalo it may, Beach & Ransom (20) found no such effect in the rat. After satiation in the standard arena, the male was removed and either returned with the female to that same arena for further observation or tested with her in a cage of greatly differing characteristics. (He had had a 30-min opportunity to explore this cage, several hours previously.) In terms of percent of the group resuming ejaculation and the median number of ejaculations, the female was responded to equally in the two situations. Ejaculation was significantly more frequent, however, when in the second cage the male encountered a fresh partner.

The Coolidge effect has been examined as a function of interval between the end of the satiation session and the start of testing with the second female (24). The results illustrate the failure of different "measures of drive" to covary. As the interval is increased from 0 to 24 hr, initial intromission latency increases, but at the same time the refractory period between the end of the first ejaculation series in the test and the beginning of the second decreases. The assumption that both of these reflect (inversely) the level of sexual arousal must be re-examined. It is conceivable that the discrepancy results in this experiment from interaction with the circadian variation in responsiveness (see 131).

Extraneous stimulation.—The concept of novelty must come to be more precisely specifiable in stimulus terms before it can help us differentially predict facilitation or distraction. Similarly, "handling" has different levels of meaning, as investigators of the experience-emotionality relationship have discovered, and one experiment indicates that it merits equally careful

analysis here. The waning sensitivity of the aging male rat is in some respects revived by picking him up for a few seconds a couple of times each minute during the mating test, a procedure that has no influence on young Ss' performance (132). How this functional rejuvenation is related to novelty and ultimately to arousal theory is not yet clear, but its description has led quite directly to experiments which seek to modify coital pattern by extraneous electrical stimulation.

Males selected for sexual vigor were given four weekly mating tests, with electrodes attached to the back (Barfield & Sachs 9). During the experimental sessions 0.5-sec shocks were given automatically every 30 sec. Control sessions (first and third in the sequence) differed only in that current was not applied across the electrodes. In contrast to the random distribution during the nonshock tests, in the stimulation sessions 72% of the copulatory responses occurred within 5 sec after shock delivery. There resulted, in other words, a pacing of the sexual behavior, so that the peak of intermount intervals was at about 30 sec. Continuation of the shock administration after ejaculation decreased by about 35% the delay before initiation of the next copulatory sequence.

Facilitation of responding occurs also in sexually inexperienced Ss (Caggiula & Eibergen 41). Tail shock was given at the beginning of the test session and every 60 sec thereafter. The number of copulations was about four times as great as among the unshocked control group, with intromission latency inversely related to shock intensity. Shock of high intensity increased the probability of mounting when a male or a small stuffed toy was introduced as stimulus object. Loud noise, substituted for the shock, was ineffective in enhancing copulatory reaction to a receptive female.

Caggiula & Eibergen (41), but not Barfield & Sachs (9), report a few instances of aggression precipitated by the shock. With male-female pairs this never took the form of the complete fighting pattern that has been extensively investigated by Azrin and his co-workers (see 43); the "confrontations were always exceptionally brief and were frequently followed by mounting" (41, p. 416). One pairing of males did lead to stereotyped fighting, but after some guidance by the experimenter it was replaced by mounting and thrusting. Whether mating or aggression occurs in response to shock may depend upon not only its parameters, including point of application and intensity, but also the various social, experiential, and physical characteristics of the situation (cf. 84, 163).

Patterning of copulatory sessions.—The frequency of the various components of the rat's mating pattern and the intervals between them are often considered to reflect the male's level of sexual excitement, and in turn the excitement level is regarded as a function of feedback stimulation from each active component. Change in pattern resulting from various types of experimental intervention may help clarify these relationships.

The longer the delay enforced between successive intromissions by

separating the partners (at least up to 2–3 min), the fewer the intromissions required for achievement of ejaculation. One way in which reduced number of penile-vaginal contacts could generate sufficient stimulation to exceed the ejaculation threshold is prolongation of each contact. Just such a change in intromission characteristic has been recorded: from an average of 265 msec during self-paced copulation, to 331 msec at interintromission delays of 5 min (23).

Another technique for modifying the pattern of feedback is to prevent intromission by suturing the vagina (98). During 40 min with such a preparation, the median experimental male mounted 37 times. This provided too little accumulating excitement to lead to ejaculation, but subsequent events indicated that an appreciable degree of arousal (from stimulation or from frustration?) was developing. When immediately after this bout a normal female was provided, the male ejaculated after only 1 nonpenetrating mount and 6 intromissions, compared to control medians of 4 and 8; ejaculation latency was 40% shorter in the experimentals than in controls.

The amount of stimulation received by the female rat during sexual contact affects her subsequent behavioral responsiveness and her physiological reactions. It is well documented that in a situation in which the estrous female can bar-press to gain access to a male, the rate of her pressing is inversely related to the amount of just-preceding stimulation—fastest following a mount without penetration, of intermediate speed after an intromission, and slowest after ejaculation. Bermant & Westbrook (26) further varied the stimulus intensity. In some sessions they prevented the expulsion of semen during the male's ejaculatory reflex by sympathetic blockade; absence of the plug led to more rapid response by the female than when a plug had been deposited. In another experiment they compared normal response times with latencies when vagina and perineal area were topically anesthetized. Responses following each type of male reaction were faster than normal under the condition of chemically reduced stimulation. Accepting a straightforward drive-speed interpretation, it would follow that during a certain stage of the mating sequence the net effect of each sexual contact is to reduce the female's drive level, the degree of reduction being directly related to amount of sensory feedback. Consistent with this is the contrast between the "frantic" (presumably strongly motivated) behavior of the bitch following premature termination of the genital lock (or after insertion without lock) and her markedly reduced responsiveness immediately following the more intensely stimulating lock of normal duration (15).

Physiological processes that increase the likelihood of pregnancy or pseudopregnancy are in the rat under partial control of amount of stimulation resulting from intromission (1), with neither mounting nor stimulation by the seminal plug critical. Fertilization of the eggs is itself dependent on multiple intromission (1), as is mammary gland development (64). Comparative studies may illuminate the reciprocal relation between the response-dependent stimulation and hormonal state. For example, the corpora lutea

of chinchilla (and guinea pig) develop and produce sufficient progesterone to insure implantation and successful pregnancy, without the considerable cervicovaginal stimulation required in the rat (mouse, hamster); the female normally permits a much smaller amount of sexual contact in chinchilla than in rat, and the male chinchilla ejaculates after only a few seconds of thrusting (28; see above).

Characteristics of the partner.—Female dogs are more likely to be mounted by other females or by males (as well as to mount males) when they are in heat than when they are anestrus (21). To relate variation in stimulus value more precisely to hormonal state, Beach & Merari (18) observed the responses of male beagles after the bitches had received two injections of estradiol cypionate 48 hr apart, after another injection of the estrogen 3 days later, and after an injection of progesterone 6 days later. In the days following estrogen treatment the males were attracted to caged females with increasing frequency, they vigorously sniffed cotton balls with which the vulva of the females had been swabbed, and their mounting scores gradually rose. When progesterone was added, cage visitation and sniffing remained unchanged, but mounting scores increased by another 100%, and licking and chewing of the cotton balls (and licking of the genital region when accessible) appeared for the first time. The increment of mounting observed during the progesterone stage can be understood in terms of the rising drive level of both partners, dependent on the enhanced mutual stimulation, gustatory and tactual.

The response of the spontaneously estrous bitch to the attracted male ranges from cooperation through permissiveness and passive avoidance to attack. Repeated tests pairing males and females that had lived as a group for several months revealed not only that some males are generally more acceptable as mates than others, but that a given female may be idiosyncratic in her pattern of choices and accept most often the male regularly rejected by the others (16). [Hart (102) observed no such differential preference, but his Ss were spayed bitches brought into heat with estradiol cypionate, and had had no opportunity for acquaintance with the males outside the testing situation.] Acceptability as a sex partner is apparently uncorrelated with either dominance in a food-competition situation or more general interindividual relationships ("social affinity"). A particular male may rank last in affinity for an anestrus female, but first in mating acceptability during her estrus. To what extent the reversal of ranks is due to a change in the bitch's attitude when she comes into heat, or to individual differences in the males' response to the estrous female (and hence their relative acceptability) has yet to be ascertained.

Activation and reinforcement of other behavior.—Are forms of behavior other than sexual increased during the female rat's estrus? Moss (154) recorded bar-pressing, noncontingent or (in some experiments) followed by 3-

sec light change or buzzer, during daily 10-min periods. Fluctuations of bar-press duration, but not of frequency, regularly paralleled the estrous cycle, with peaks coincident with greatest receptivity and vaginal proestrus-estrus. No consistent cycling was observed in males, ovariectomized females, females during prolonged vaginal estrus or during the diestrus of pseudo-pregnancy. Further, restoration of receptivity in ovariectomized Ss by estrogen-progesterone therapy was accompanied by an increase in bar-press duration.

Bolles, Rapp & White (34) found that females brought into behavioral estrus (receptivity) by estrogen-progesterone traversed a 155-cm runway more rapidly than diestrous controls, with a male in the goal box. While elevated sex drive might by this measure be regarded as energizing, its reduction by sexual contact seemed not to be reinforcing. Running time dropped over a half-dozen trials spaced 10 min apart, but the improvement was the same for those Ss encountering a sexually vigorous male and those whose incentive male was sexually unresponsive.

The opportunity to achieve intromission has been shown to be an effective reward for performance by the male rat in a T maze (214). Ware (212) has suggested that the differential reinforcing value of the several degrees of sexual response should be relative to an "internal standard" or adaptation level based on prior experience. Thus, for mature but sexually naive individuals intromission without ejaculation might be no less reinforcing than intromission with ejaculation, but after having had experience just with intromission a rat would be reinforced more by ejaculation than by mere intromission, and least of all by exposure to an unreceptive female. To test this formulation, three groups first were run for several trials (6-ft alley) to a diestrous female, to an estrous female with which two intromissions were permitted (insufficient to reach ejaculation), or to a receptive female with which intromission and ejaculation occurred. Then each group was divided into three subgroups ($n=3$), partitioned respectively among the three incentive conditions. Those Ss allowed ejaculation in Phase II were fastest in both running and mounting, and those given the unreceptive companion were the slowest. More important, when the shift in reward between Phases I and II was in the direction of more complete sexual response (e.g. from no intromission to intromission), behavior was facilitated, while the shift toward less complete response (e.g. from ejaculation to intromission) led to retardation. It would appear that adequate description of the reinforcement potential of sexual behavior must include some consideration of adaptation level.

CONTROL OF INGESTION

The literature on food and water intake is continuing to gain weight rapidly. The 1969 and 1970 meetings of the Eastern Psychological Association each devoted four sessions to ingestion. Following the appearance of a *Handbook of Physiology* (48) volume devoted entirely to the topic, the New

York Academy of Sciences (151) has published the report of an interdisciplinary conference on the neural regulation of food and water intake, in which 60 investigators report recent developments in their work. A symposium on the chemical senses and nutrition (119) contains discussions of many pertinent issues. Finally, Morgan & Jacobs (152) have written a review of hunger and satiety which, while one may disagree with some of what they say, is a valuable guide to the diffuse and scattered literature on the problem.

Control of water intake.—It is now well established that water intake is under multiple control. Rats drink in response to an increase in serum effective osmotic pressure (e.g. 54), and they also drink in response to a reduction in extracellular fluid (ECF) volume (e.g. 192). The two factors appear to be independent and additive (cf. 31, 53).

The neural mechanisms of volemic and osmotic thirst are separable. In rats with extensive damage to the frontal-pole area, Blass (29) showed a selective depression of thirst induced by osmotic stress (hypertonic saline injection), but no disruption of normal water intake or of drinking following ECF volume reduction. Blass' lesions involved extensive damage to the lateral preoptic area, and he went on to show (30) that cells in this region may be receptors for osmotic thirst. Lesions here disrupted osmotic, but not volemic, thirst; and local osmotic stimuli, applied through indwelling canulas, had powerful effects on drinking.

Whereas the brain seems to monitor directly the osmolarity of the fluid perfusing it, volemic thirst involves a peripheral loop. The effect of ECF reduction on water intake is diminished by nephrectomy, though not by ligation of the ureters (cf. 31). In an elegant series of experiments, Fitzsimons (76) has presented strong evidence that a blood-borne dipsogenic factor, probably renin, is released by the kidney in response to reduction in its local blood flow (which normally accompanies general ECF reduction). Renin in turn acts on a substrate in the blood to release angiotensin, and Epstein, Fitzsimons & Simons (68) have evoked drinking with microinjections of angiotensin in various brain areas.

Fitzsimons notes that while the effects of systemic volume reduction on drinking are reduced by nephrectomy, they are not abolished. Some other, extra-renal, factor must also respond to hypovolemia by inducing thirst. Thus the fractionation is proceeding: it seems that the volemic component of thirst is itself a multiple-control system. Pressure receptors in the vascular system are a likely candidate for the missing control.

Rats in which thirst is induced by ECF reduction typically stop drinking water long before the ECF volume is restored to normal. Stricker (193) has concluded that such thirst is satiated by the dilution of the body fluids which ingestion of water produces. He points out that, since drinking evoked by a particular deficit can be satiated without the removal of the deficit, an active inhibitory process must be involved in the satiation. Cells

in the septal area may contribute to such a function, since lesions in this area produce a selective enhancement of drinking in response to volemic thirst (32).

Both osmotic and volemic thirst are permanently abolished by lesions in the lateral hypothalamic (LH) area (70, 195). Nevertheless, as food intake recovers, rats with LH lesions eventually recover normal levels of water intake and maintain themselves on ad lib food and water. Is there still another control for regulatory drinking?

Not necessarily. Such "recovered" rats drink only when they eat, taking repeated small draughts of water during a meal rather than large draughts before or after it, as the intact rat does. This "prandial drinking" pattern develops in desalivate, neurologically intact rats as well as in LH rats, and it seems to be related not to bodily water needs, but to the mechanical problem of swallowing dry food with a dry mouth. Thus intragastric injections of water do not suppress drinking in "prandial drinkers," but injections into the mouth do (123, 124). Finally, since desalivation enhances water intake in the LH rat (if food is present), and is the only known dipsogenic stress which does so, Kissileff & Epstein (126) conclude that the LH rat is exclusively a prandial drinker: it drinks in order to swallow dry food, thereby, quite coincidentally, meeting its water needs.

Kissileff is careful to point out (124) that these results are not support for a dry-mouth theory of thirst. Prandial drinking is not the result of a dry mouth per se (desalivate rats drink less than normal ones if food-deprived) but depends on the presence of food; and there is evidence that it is learned (123). Strictly speaking, then, drinking of this type is not an expression of thirst at all; it is motivated by hunger. It is an operant, which "enables the animal to swallow dry food, just as bar-pressing serves as an operant for obtaining food" (123, p. 299).

Two types of thirst, then, and a pseudotype. But this is not the end of complications. Fitzsimons & Le Magnen (77) report provocative evidence that rats may learn to anticipate impending water requirements. Their rats did most of their drinking in close association with meals, except for a period following the transition from a carbohydrate-rich diet to a protein-rich one, which obligated more water for excretion. During that transitional period, total water intake rose, but much of the excess water intake occurred between meals, perhaps in response to the increased postprandial water loss. Later, however, the animals' drinking moved back into close association with meals, while remaining high—as if the rats had learned that the diet now offered required a higher water intake, and were meeting future water requirements long before they were actually incurred. The idea that a rat might learn how much water it is *going* to need would once have seemed incredible, but our present knowledge about dietary preferences and aversions suggests that a rat may well have just such capabilities (see below).

As the identifiable factors controlling drinking multiply at an alarming rate, it is good to be able to reduce them by one. Rats increase their water

intake in the heat (cf. 70), and the possibility that hyperthermia may stimulate thirst directly has often been advanced (but see 187). Hainsworth, Stricker & Epstein (93) have concluded that in the rat such drinking is secondary to systemic dehydration. Rats keep cool in the heat by spreading saliva over their coats, paying for the evaporative cooling with a substantial water loss (92). Drinking in the heat begins only after dehydration has been so incurred, and if it is prevented by desalivation, drinking is abolished. Therefore, heat per se is not an adequate stimulus for drinking, at least in the rat.

Glucostatic control of feeding.—Hunger, like thirst, is a multiple-control system. This was shown directly by Epstein & Teitelbaum (71), who found that rats with LH lesions, having recovered feeding and drinking, fail to eat in response to insulin-induced hypoglycemia. The finding shows first that the feeding response to low blood sugar is a separable component of hunger; and second that it is not a necessary component (rats which displayed this deficit were otherwise feeding normally).

How this mechanism works has been a subject of continuing investigation. There must be cells which monitor some parameter or correlate of blood-sugar level, but what they are monitoring is not clear. The relation between feeding and blood glucose level, or arteriovenous glucose difference (which reflects glucose uptake by the tissues), is inconsistent at best (cf. 175, 183). But this may mean only that other mechanisms obscure the relations that would obtain if the glucostatic system were operating alone. More illuminating have been the attempts to influence glucose metabolism by experimental intervention.

Smith & Epstein (183), for example, have induced feeding in the rat and monkey by injection of 2-deoxy-D-glucose (2-DG), a glucose analog which blocks glucose utilization in certain organs such as brain and liver. As a result of the blockage, blood sugar rose; nevertheless, the animals ate. This finding is a strong addition to the evidence (cf. 183) that absolute blood sugar level is not the critical factor.

At present it seems most likely that the rate of glucose utilization is monitored. 2-DG is assumed to block that utilization directly, while insulin does it indirectly by inducing hypoglycemia, thus making glucose unavailable to the receptors. It follows that the latency of feeding should be longer after insulin than after 2-DG injection; and this is so (183). Booth & Pitt (38) report that insulin-induced feeding begins only after the net uptake of glucose by the cells has fallen to a low level. On other grounds, however, Booth and his colleagues argue (36, 38) that the effect of insulin cannot be accounted for solely in terms of its effects on glucose metabolism; for example the feeding response to insulin (but not the hypoglycemia) is blocked by puromycin, a protein-synthesis inhibitor (36). (For further discussion of this complex issue, see 48, Ch. 28; 82, 121, 184, 188, 189.)

The anatomical locus of the glucoreceptors is also uncertain. As Epstein

& Teitelbaum point out (71), their ablation studies do not imply that LH cells serve receptor functions; these tissues may be involved in transmission of information provided by receptors elsewhere. The same may be said of the finding that single-unit activity in the hypothalamus changes with intravenous glucose infusion (4). In the mouse, there is much indirect evidence that glucose is taken up in large quantities by cells in the ventromedial (VM) area, and that insulin plays a role in the uptake (cf. 60, 146). But a high rate of uptake does not necessarily mean receptor action.

Clear evidence for receptor action could be obtained if one could mimic the effects of natural stimuli in the local environment of the cells involved. Some hypothalamic units respond to microinfusion of glucose through a barrel adjacent to the recording micropipette (160). Booth (35) has reported that microinjections of glucose into LH depress feeding elicited by systemic insulin. However, the cerebral injections produced signs of drowsiness, so this inhibition of feeding may be artifactual. Of course, even if a drop in glucose utilization rate is involved in the initiation of feeding, it does not follow that an increase in such utilization is a sufficient condition for its termination. It would be interesting to know whether one could elicit feeding by local blockage of glucose uptake in the hypothalamus, perhaps by injecting 2-DG into the brain.

In short, while it is highly likely that there are glucoreceptors in the brain (e.g. 160), it remains to be demonstrated that they act to control feeding. Moreover, there may be glucoreceptors outside the CNS—e.g. in the liver (159). In dogs, intraperitoneal glucose produces a more profound anorexia than does intravenous injection, perhaps because the former is absorbed into the portal circulation and taken up by the liver (175). In cats, the period of anorexia which follows intraperitoneal or intraportal glucose injection corresponds to the period of hepatic glucose uptake (169). These liver receptors, then, may well play a part in controlling feeding. Indeed, we must be prepared for the possibility that both central and peripheral glucosensitive cells are involved. In view of the marked differences between the central and the peripheral effects of insulin (cf. 40, 79), this would make the task of analysis particularly challenging.

The regulation of meals.—In 1966, Le Magnen & Tallon (139) reported an observation which has had wide ramifications. In free-feeding rats, the size of a meal determines the interval between that meal and the next one: the larger the meal, the longer the rat waits before eating again. The size of a meal, however, bears no relation to the length of time the rat waited before eating.

There are two ways of interpreting these relationships. One (48, Ch. 2) is that the cycle of hunger and satiety is determined by a single "satiety" mechanism, so that feeding is released when an internal "satiety" signal falls below threshold level. The larger the preceding meal, the longer that will take. But once feeding is released, its consequences will raise the "satiety

signal" back above threshold at a rate independent of how long it took the signal to reach threshold and initiate the meal. DeRuiter, Weipkema & Reddingius (61) have explored some consequences of a theory such as this.

The other possibility (e.g. 5) is that different factors in satiety operate sequentially. Feeding may be terminated by events which occur during the meal and are independent of the state of voluntary deprivation at the time the meal began. Once feeding is terminated, however, its inhibition is maintained by longer-lasting internal mechanisms, the effects of which are more prolonged after a larger meal. Such a mechanism would account for the fact that rats do not quickly regain their weights after a longer-lasting, imposed food deficit (3). Dogs do (cf. 3); one wonders if their feeding patterns would show the Le Magnen-Tallon relations.

A report by Davis et al (59) may be construed as support for a multi-stage system. Mixing the blood of fasted and sated rats depressed subsequent feeding in the former by about 50%. The puzzling finding is that whereas the hungry rats' feeding was depressed by mixture with ad lib rats' blood, intake was still substantial. Yet the sated rats ate little or nothing, despite blood composition presumably identical with that of the hungry rats, which did eat. A possible explanation—there are others—is that the humoral factor implicated here is involved in the short-term satiation of feeding (hence its suppressant effects in hungry rats), but is not necessary for maintained inhibition (hence the ineffectiveness of its dilution in sated rats). Davis et al (59) suggest that the active substance may be a gastrointestinal hormone, and they mention enterogastrone as a possibility.

The Le Magnen-Tallon effect is observed also in bar-press situations (8). Using such a method with liquid diet, Thomas & Mayer (203) showed that the relations hold for both normal and hyperphagic rats, though the latter ate much the larger meals. When slow, continuous intragastric (IG) infusions of diet were added to the rats' spontaneous intake, that intake declined (though not enough to compensate for the amounts infused), primarily through a reduction in meal frequency; meal size was much less affected. In contrast, when the IG infusions were made part of the meal (each bar-press delivering both an IG injection and a quantity of fluid to be drunk by mouth), oral intake was again reduced (though again, not enough), but this time the primary effect was on meal size, not on meal frequency. Therefore, relatively large amounts of IG food delivered with the meal contribute to the termination of that meal; slow, continuous infusions do not, but they do contribute to the longer-lasting inhibition that governs inter-meal interval.

Since injections of water or of isotonic saline with meals had no effect on ingestion, gastric distension and osmotic shifts of water must play at most a minor role in meal termination. Some correlate of the energy value of the food must be sensed directly. And since meals lasted only 2–4 min, presumably too short a time to permit appreciable absorption (but see 190), gastrointestinal chemoreceptors may be involved.

Snowdon (186) has extended the analysis to feeding without oropharyngeal sensations. Rats pressing a lever for IG injections of liquid diet were given control over meal size by providing for continuous infusion as long as the bar was held down. When the food bypassed the oral cavity, the rats took in consistently less food and ceased to gain weight; so the motivating properties of taste and smell do contribute to the maintenance of normal intake. Still, there was regulation. The Le Magnen-Tallon relations held true with IG feeding of full-strength liquid diet. When the diet was diluted with water, the rats increased their intake, primarily by increasing meal size. We thus have the converse case of, and good agreement with, the Thomas & Mayer (203) findings. However, the adjustment took four to six meals to complete, so more is involved than a direct control of sensed nutrient content over each individual meal.

Snowdon concludes that oropharyngeal factors are not necessary for the essentially normal control of total intake and of meal distribution. The question has been raised, however, whether the IG-feeding situation is really free of oropharyngeal influence. Snowdon reports some difficulty in maintaining bar-pressing for IG reinforcement, and observed in addition that his rats made licking, chewing, and swallowing movements while feeding IG. Holman (111) has asked whether the IG-reinforcement effect depends upon the feedback from such acts and other oral sensations such as the thermal stimuli resulting from the passage of chilled diet through a nasogastric tube [in both Epstein and Teitelbaum's (69) experiments and in Snowdon's, the diet was delivered to the animal from a refrigerated reservoir]. In rats bar-pressing for IG diet, he showed that extinction of the bar-press was likely unless the diet were cold and were delivered through a nasogastric tube; warm diet, or diet delivered to the stomach by a subcutaneous route, was much less effective in maintaining behavior. These and other data led Holman to suggest that the IG injections act by enhancing the reward value of oral stimuli, even if these are not specifically gustatory (e.g. thermal stimuli).

This is a worrisome set of findings. It is unfortunate that Holman never repeated exactly the procedures of other users of the method. His rats were permitted access to the bar-press situation intermittently, not continuously, and the volumes he injected with each response (1 or 2 ml) were quite small. Nevertheless, the effects of his manipulations are impressive; he is able to recondition bar-pressing, after it apparently has quite extinguished, by the simple expedient of chilling the diet. Certainly his findings point to a possible artifact which other investigators should consider.

The powerful methods developed to assess the contributions of oral and postingestive factors in animal feeding are now being applied to man (117, 118, 211; see also 198). Subjects who could not see the food they were ingesting showed reasonably stable intakes over several test breakfasts, even though they were very poor at judging the amounts ingested. When they fed by activating a pump which delivered liquid diet IG, most of them

underate. But they ate too much when diet was delivered simultaneously to the mouth and the stomach; there was some reduction in oral intake, but not enough to compensate for the added calories delivered IG. In man then, as in the rat, regulation can take place without oral cues, but the latter have a potent effect if present. The postingestive cues by which humans regulate meal size seem to be based on volume, not calories: dilution with water of the diet injected IG led to little or no compensatory increase in total volume intake during a single meal (117).

Jordan (117) extended the investigation to a 48-hr period during which his Ss were permitted three meals a day by IG feeding. All of them fed adequately and maintained their weight, and their breakfast intakes on the second day were within the range of prior breakfasts.

Finally, we may note that in hunger as in thirst, anticipatory factors may influence a bout of feeding. Artificially induced anorexia can be conditioned (138, 175), and some systemic changes associated with the consequences of feeding, such as changes in blood-sugar level, are conditionable (7, 37) and occur very early in a feeding sequence (158).

Regulation of body weight.—To the extent that an animal regulates its energy balance, it must vary its caloric intake not only in response to short-term challenges of the kind we have been discussing, but also in appreciation of the variations in energy output which its environment requires. And there is good evidence (see 121) that day-by-day intake regulation is superimposed on a long-term mechanism which, despite wide transient fluctuations, maintains energy balance as reflected in body weight with remarkable precision.

Kennedy has suggested (see 121) that energy balance is maintained indirectly by regulation of depot fat, the content of which will reflect any persisting difference between energy intake and output. This "lipostatic hypothesis" implies that the animal has a way of monitoring its own fat stores. Hervey (107) has suggested that an animal might measure its own stored fat just the way an investigator might measure it *in vivo*—by dilution. If a substance were elaborated and disposed of by the body at a constant (or at least known) rate, and if this "tracer" were soluble in fat and in plasma and partitioned between the two in a fixed ratio, then its concentration in the blood would provide an inverse measure of the amount of stored fat in the body: the higher the level of circulating "tracer," the less fat.

Without attempting further identification of the "tracer," Hervey suggests that steroids have the requisite characteristics. And it is well established, in large part through the work of Hervey and his colleagues, that changes in energy balance can be forced by administration of steroid hormones or removal of their sources (see 107, 210 for references).

However depot fat is monitored, information about it must be translated into changes in behavior, presumably by way of the brain. Hoebel & Teitel-

baum (109) present compelling evidence that a disorder in weight regulation is an important component of hypothalamic hyperphagia. Following VM damage, rats would not overeat if they were already obese. And if, having ceased to gain weight at high levels of obesity, they were made "super-obese" by force-feeding, they would then voluntarily reduce their food intake and lose weight, returning to the plateau that had obtained before force-feeding was imposed. This is precisely what normal rats do in defending their normal, lower body weights.

In a sense, then, the term "hypothalamic hyperphagia" is a misnomer: overeating is not a necessary consequence of effective VM damage. Hoebel & Teitelbaum suggest that overeating occurs only because the weight level which is to be defended has been elevated by the brain damage, perhaps because of reduced sensitivity to some "tracer" correlated with fat stores. The rat overeats, in other words, because of a sensed state of low body weight. Thus, while food intake of course affects body weight, it seems also that weight or its correlate, as sensed by the controlling system, acts back to influence food intake; and the conditions for a closed-loop "lipostatic" control are met.

In contrast to the effects of VM damage, lesions in the LH are well known to produce aphagia and adipsia, with consequent weight loss. The assumption has been that the weight loss is secondary to disruption of the feeding and drinking mechanisms, and thus reflects a failure of regulation. This interpretation has been challenged in a provocative paper by Powley & Keesey (164), who suggest instead that the LH syndrome may involve the obverse of hypothalamic hyperphagia—a regulated weight loss. After placing small LH lesions through chronically implanted electrodes, they observed the states of recovery from aphagia and adipsia described by Teitelbaum & Epstein (202). After recovery, however, the operated rats remained much lighter than their controls; they never regained normal body weight.

The VM rat will not overeat if it is already obese. Will the LH rat still refuse to eat if already starved? Powley & Keesey reduced their rats' body weights prior to passage of current, and showed not only that the period of aphagia was thereby shortened, but also that, if starved well below the projected reduced weight level, the rat would gain weight until it reached that level. Indeed, the weight loss directly consequent to large lesions might be less than that produced by small lesions, if the rats had been severely deprived at the time the large lesions were placed.

Powley & Keesey (164) suggest that a primary effect of LH lesions is to depress the lipostatic "set point." Just as VM rats get fat because the new "set point" is too high, LH rats may starve to death because it is too low. The paper makes a strong case for body weight as a factor determining the events which follow LH lesions, but there are reasons for doubting that such a mechanism can tell the whole story. For one thing, their data seem incompatible with Teitelbaum & Epstein's (202) observation that the transi-

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tion from anorexia to regulation of intake and body weight is signaled by a spontaneous and *abrupt* rise in weight. For another, they do not speak to the problem of specificity of recovery—i.e. the recovery of hunger after LH lesions, but not of true thirst, nor of the glycemic control of feeding. Of course, these persisting deficits may be components of the LH syndrome that can be dissociated by small lesions (see 152). Also problematic are the findings (e.g. 44) that appropriately-placed lesions can produce both VM and LH syndromes in sequence: aphagia and adipsia, followed after recovery by overeating and obesity. If the animal is seeking a balance between the weight gain and the weight loss dictated respectively by VM and LH involvement, why does it not seek the final "balance point" at once? (In this connection see also 66, 67).

Perhaps this is the place to note that the resolution of such issues, when it comes, will have implications that extend far beyond the questions of intake regulation. Having demonstrated that the stages of recovery following LH lesions are closely paralleled by the development of regulatory mechanisms in ontogeny (200), and noting that the LH syndrome can be reinstated by cortical spreading depression (201), Teitelbaum, Cheng & Rozin suggest (200) that recovery of subcortical function may take place through a "re-encephalization" analogous to the encephalization that occurs as an animal develops. We are reminded of Galambos' comment some years ago that the problems of how the nervous system becomes organized embryologically, and how it becomes reorganized as a result of learning, are so closely related that "the solution to any one of these problems would mean that the answer for the others would drop like a ripe plum. . . into our outstretched hands" (83, p. 239). May we add to the list the problem of how the brain recovers its competence after injury?

Returning to the problem of weight regulation, we may note that evidence for lipostatic control, derived from investigation of brain-damaged rats, had been reported earlier. In parabiotic pairs of rats, Hervey (106) showed that the obesity following VM lesions in one member of the pair was accompanied by emaciation in the other, neurologically intact, member. Some correlate of the one rat's obesity, crossing the parabiotic juncture, must have acted to depress feeding in the other.

Not everyone has been able to replicate Hervey's findings (78, 96; but see 49). Few attempts to do so, however, have repeated his procedures. Fleming (78), for example, after placing brain lesions clearly sufficient to induce obesity in one member of his pairs, reports at most a slight depression of feeding in the unlesioned member, and no sign of emaciation. But the rats had access to food for only 10 hr a day; and a deprivation schedule, even if it will support obesity, pits the short-term state of hunger against the long-term depressant effect of obesity which Hervey saw. Fleming does report one parabiotic pair in which Hervey's findings obtained. Of this pair, the VM obese member was female; the emaciated pair-mate, male. No further breakdown by sex is reported.

In this connection arises a line of investigation that should lead to sub-

stantial advances. Cox, Kakolewski & Valenstein (56), noting a variety of discrepant results in which subjects of different sex were involved, have compared the effects of VM damage in male and female rats. The resulting increase in food intake was much the greater in females, and only females gained weight.

It is likely that this sex difference is a quantitative one, for hyperphagia has been demonstrated in male rats (e.g. 106); but perhaps it is more easily obtained in females, or greater in magnitude. At this point, however, we should mention a potential artifact which this investigation shares with many others in the hyperphagia literature. Cox, Kakolewski & Valenstein (56) offered their rats only one diet: powdered laboratory chow. This diet seems not to be very attractive to rats; and Corbit & Stellar (55) have shown, and we have seen repeatedly in our laboratory, that many rats with brain damage sufficient to produce marked hyperphagia when more attractive diets are offered, simply do not express hyperphagia when offered this one. Failure to consider this aspect of the phenomenon is widespread, and may in part account for the feeling that "the production of obese rats by neurological damage must be an art rather than a science" (170, p. 569). And it raises a question about the findings of Cox and his associates (56): There are sex differences in preference behavior (209). Could there be sex differences in "finickiness"?

This comment is not intended to minimize the significance of the findings by Cox et al, for clearly there is *some* difference between males and females in their response to VM lesions, and the difference is important. Valenstein and his colleagues (207) review the evidence for disruption of gonadal function following VM damage and note a similar sex difference in the response to gonadectomy: female rats increase their rate of weight gain, males do not (see also 162). In this connection, we have observed (150) that the hyperphagia and weight gain which otherwise follow VM damage in the female rat can be suppressed by exogenous estrogen.

Our discussion of weight regulation thus has come full circle: from the possible role of steroid hormones as depot-fat "tracers," to the influence of the brain on regulation of weight, to an interaction of brain mechanisms with the endocrine system. This interaction, as it affects energy-balance regulation, is likely to receive a great deal of experimental attention over the next few years.

Preferences and aversions.—In previous volumes of the *Annual Review of Psychology*, the topics of preference, aversion, specific hungers, and the like, have been treated in chapters on the chemical senses—and appropriately so, for taste and smell are powerful determinants of preference behavior (39, 148). It is also true, however, that preferences are closely linked to regulatory and motivational systems, and the distinction between "regulatory" and "nonregulatory" aspects of preference is becoming increasingly hard to draw.

In the rat, for example, Smith & Stricker (181) have shown that the fluid

an animal chooses to drink depends upon the reason for its drinking. Given a choice between water and an isotonic saline solution, rats drinking in response to volemic thirst chose the saline; rats drinking for osmotic reasons, water. These are wise choices. Plain water will restore a cellular hydration deficit more efficiently than would the saline solution, but the saline will more rapidly restore a plasma volume deficit.

In fact, a substantial reduction in plasma volume cannot really be made good until sodium, as well as water, is restored to the body fluids. And such a plasma reduction is accompanied by a salt appetite which is clearly distinguishable from the thirst that also is evoked (cf. 196). The salt appetite is much slower to develop. It is potentiated rather than satiated by body-fluid dilution (194). And whereas hypovolemic thirst can be alleviated by restoration of the plasma deficit, salt appetite persists long after sodium balance has been restored (115). Stricker & Wolf (cf. 196) have suggested that the stimulus for this salt appetite is the depletion of a sodium reservoir, perhaps in bone. Such a mechanism accounts for the time course of the appetite, since mobilization of reservoir sodium to combat a systemic sodium deficiency, and repletion of the reservoir after the deficiency is made good, both take time (but see 217).

Evidence is accumulating that salt appetite is mediated by a change in responsiveness to the salt taste (148, 182, 185). Wolf has reported (216) that the salt appetite evoked by Formalin injection is impaired following lesions in or posterior to the gustatory thalamus. He suggests some pathways by which taste input might communicate with the hypothalamic systems which other work (e.g. 165) has implicated in the control of salt intake.

The specific salt appetite observed following interference with the body fluids is superimposed on a "spontaneous" salt preference, displayed by rats in no known state of imbalance; and this preference too is largely taste-determined (39, 148), though only where selection is involved (148, 149). Interest in the neural substrates of this preference has increased over the past few years, and a number of investigators (e.g. 6, 45, 46, 89, 125, 176) have reported that CNS manipulations can affect it. It must be said, however, that many of these studies are rather crude on the behavioral side: the conditions under which a preference disorder occurs, apart from locus of stimulation or ablation, often are explored hardly at all. Kissileff's work (122, 125) is an exception, and he has pointed out clearly the sort of pitfalls which abound. Rats which had recovered feeding and drinking following LH lesions failed to express a salt preference (125). This apparent preference deficit, however, may really be a failure of peripheral stimulation. Since such rats drink only when they eat, the taste of the saline solution may be masked by the taste of food in the mouth. In surgically desalivate rats (122) or in intact rats treated with atropine (136), the salt preference is lost as the "prandial drinking" pattern develops, but is promptly restored by withdrawal of food.

Regulated intake of specific dietary commodities extends to food substances. Rozin (172) has found that the intake of protein may be regulated separately from the ingestion of other nutrients. A sucrose solution, a fluid which is also a food, may be treated as food or as fluid depending upon the rat's state of need (51).

Continuing his studies of the role of learning in specific hungers, Rozin (173, 174) has further supported his contention (cf. 48, Ch. 30) that a specific kind of learning mechanism is involved (see also 128). Vitamin-deficient rats, offered a choice of diets including a vitamin-rich one, learned to choose the "good" diet only after they had taken a large and well-isolated meal of "good" food (173). The beneficial consequences of eating "good" food are presumed to educate the rat most effectively if they have time to occur before the ingestion of other foods confuses the situation. Rozin suggests that the rat's lesson may be made easier to master by the anorexia which accompanies vitamin deficiency: a rat which eats little is likely to space its meals well apart.

Implied in these conclusions is the idea that rats are capable of associating a food with the long-delayed consequences of ingesting it (see 48, Ch. 30). Rozin (174) has shown that malaise induced by apomorphine, injected as long as 30 min after ingestion, can affect subsequent responsiveness to the substance ingested. Rats would differentiate between the solution which was followed by sickness and one which was not, even when the two consisted of saccharin at different concentrations, so that such factors as aftertaste could not have bridged the delay.

It seems that learned preferences and aversions of this kind must share with natural ones a dependence on taste as a means of selection, since such learning is possible even when non-nutritive, nontoxic saccharin is used as stimulus material, and when the only "consequence" of ingestion is exogenously determined. Jacobs & Sharma (114) have offered an interesting theory about the role of taste and related factors in regulation, an account which may establish new links between the analysis of preference and that of hunger and thirst. Their idea is that as an animal's hunger increases, palatability factors take on increasing importance in dictating its response to potential foods. The result is that the animal with food available eats for calories, but the hungry animal, which really needs calories, eats for taste. They back up the suggestion with a series of provocative experiments: for example, food-deprived dogs were found much more reactive than non-deprived ones to saccharin sweetening of their food.

The suggestion of Jacobs & Sharma (114) would appear directly counter to common sense, but it is not. If taste is the animal's major means of identifying food, and if a good taste usually means nourishing food, it seems reasonable that the hungry animal should be particularly responsive to a sweet taste. An open discussion, which followed Jacobs & Sharma's presentation of their paper at a conference, appears with the published version and is a good introduction to the complex ramifications of their ideas (see also 218).

GENERAL ACTIVITY

We will confine our follow-up of Campbell & Misanin (43) to just a few aspects of general activity which they discussed. A useful supplement is a review by Collier (50).

Neural mechanisms.—Since general activity varies with a number of physiological manipulations, and since a considerable number of brain areas have been implicated in the fluctuating drive states of experimental animals, it is not surprising that over the years changes in activity level have been produced by stimulating or ablating numerous sites. Such demonstrations continue, without necessarily clarifying the relationship between drive and activity. Damage to the lateral and medial septal nuclei (65), or principally to the latter (47), was followed by 50% reduction in the rat's wheel running, giving no support to the heightened-drive explanation of other behavioral changes in septal animals. In contrast, removal of the greater part of the hippocampus (usually with some ventral sparing) either had no significant effect on activity [wheels (197)], or increased it [tilt cage (178, 197), direct observation (116)]. Increased sensitivity to stimulation (65, 178), or slower habituation to stimulation (197), is the preferred explanation.

Rosenquist & Hoebel (171) found that electrical stimulation of points in the lateral hypothalamus associated with both self-stimulation and stimulation-bound eating greatly elevated wheel running, but only when food was unavailable. Since food-deprived rats also run more than normal, but shift to eating when possible, these authors suggest that feeding and running may be related at this neural level. But while an hypothesized mechanism controlling locomotion may well interact with a feeding system, the two are not coextensive, for stimulation of other points in the lateral hypothalamus and in the septal region provoked large running increments but no eating (see also 48, Ch. 25). Because all "running sites" overlapped with self-stimulation sites, Rosenquist & Hoebel propose a relationship between the activity system and reward mechanisms. Gerben (87) obtained an intensity-related enhancement of running with posterior hypothalamic placements, but no test of eating or self-stimulation was made.

It has frequently been noted that the effects of drive variables upon wheel running are not always duplicated with other "measures of general activity." Attempts to account for the differences have seldom gone beyond vague allusions to sensory feedback, inertia, specificity of reinforcement contingencies, and the like (213). Particularly welcome, therefore, is the demonstration by Campbell & Lynch (42) that these discrepant responses are differentially modified by lesion. Frontal-pole ablation greatly increased wheel running but had no effect on stabilimeter scores under conditions of ad libitum ingestion. Food deprivation augmented operated rats' activity in the wheel by only normal amounts, but increased stabilimeter scores dramatically more than in intact rats [as does amphetamine (143)]. Since

bar-pressing for food was reduced by the extirpation, enhancement of the incentive-motivational properties of deprivation seems to be an inadequate explanation for the potentiation of stabilimeter responses. Campbell & Lynch attribute it rather to blocking of "the normal cortical inhibitory reaction to changes in the reticular activation system" (p. 21). In the wheels, running itself provides the activation that is postoperatively unchecked by frontal lobe control, while arousal in the stabilimeter awaits such an influence as food deprivation or drug. Why potentiation fails to occur under water deprivation is not clear.

In apparent contradiction, Harrell & Isaac (101) found that deep frontal lesions did produce increases in stabilimeter scores without the imposition of food deprivation. Whether the disagreement is based on difference in apparatus, in testing schedule (1 hr/day vs continuous recording), or in type of tissue destruction, is uncertain. The much greater activity increment in light than in the dark leads Harrell & Isaac to doubt that the hyperactivity is a simple release phenomenon.

Lynch (142) has presented evidence that the disparities between wheel and stabilimeter responses may reflect partially separable neural mechanisms. When frontal-pole lesions are restricted to the dorsal area or when the inferior thalamic peduncle (a probable component of the reticulocortical system) is sectioned, no effect upon wheel running is observed, but the response in the stabilimeter to food deprivation is greatly augmented. Both ventral frontal lesions and transection of the medial forebrain bundle (especially if caudal) regularly increase wheel running; the effect on stabilimeter scores under deprivation shows considerable individual variation. It appears then that different telencephalic systems are involved: a relatively diffuse one (including the dorsal if not the whole frontal-pole area, the inferior thalamic peduncle, and the hippocampus) for behavior reflected in stabilimeter scores, and a more specific "cortico-hypothalamic route via the MFB by which the frontal cortex suppresses wheel running" (142, p. 57; cf. 144, p. 319).

A survey of the divergencies across the many methods of measuring general activity in the intact rat suggests that the wheel is in one category by itself, all the others in a second. The night/day, estrus/diestrus, and deprived/satiated ratios of wheel turns are much greater than those involving circular runway scores, ultrasonic cage counts, photobeam breaks, or direct observation protocols of active components of behavior, and the differences among the four are relatively minor (74, 145, 213, and unpublished data; but for a possible example of dissociation between the last two techniques, with drug administration, see 129). The wheel itself, when so altered as to require a reversal of running direction after each revolution, seems to lose its distinctive attributes and produce trends characteristic of the second category (161). Such ablations as Lynch's might provide a test of this dichotomy. If it is valid, one would expect performance in the altered wheel, for example, to respond to brain damage as do stabilimeter scores.

Food deprivation.—Why does food deprivation lead to increased general activity, especially in the wheel? Of the several candidate theories, none has yet conceded, and the eldest—which assumes gut stimulation to be crucial—has received a direct vote of confidence from Hamilton (94): “wheel running by starving rats results initially from emptiness of the gut . . .” (p. 484). The basis for this assertion is his demonstration that when the food-deprived animal is allowed to ingest non-nutritive bulk, its wheel running does not exceed the ad libitum baseline until the fifth day, by which time the deprived *S* with an empty gut is running at 300% of baseline. Hamilton cannot vote the straight single-factor ticket, however. Previous work (95) leads him to believe that in the later stages of fasting the augmentation of activity represents a defense against body-temperature reduction. Of course the activity-temperature relationship might vary with degree of deprivation. Bolles & Duncan (33) plotted the circadian rhythms of wheel running and subcutaneous temperature under the relatively mild condition of cyclic food or water deprivation. The data suggested to them that some activity increases may be related to rising rather than falling temperature.

The importance of reinforcement of previous activity in determining the level of running under food deprivation has also been debated. Connally (52) presents data supporting the negative position. Rats subjected for 21 days to 23-hr food-deprivation cycles were placed in wheels 1 hr/day on days 1–21, 5–21, 9–21, 13–21, or 17–21, just before the scheduled feeding. All increased their running over the successive sessions, but body-weight loss rather than number of preceding reinforced activity sessions appeared to be the significant variable. The results are interpreted as providing greater support for the drive hypothesis than for an explanation that involves learning. He does not account for the day-by-day increments in running that developed in an earlier experiment (75), accompanying not a loss but a 23% gain in body weight.

It has once more been asserted, by Morrison (153), that food deprivation leads not to an increased amount but rather to a redistribution of “spontaneous” behavior, with the energy normally devoted to feeding activities diverted to nonfeeding activities. Calculating from the measured respiratory exchange, he determined that overt activity (feeding plus nonfeeding) costs about 26% of the total 24-hr expenditure, whether the regimen is ad libitum solid food, liquid diet, or 1-day food deprivation. The total time during which some motor activity was recorded (by a pressure-sensitive device, with no attempt to quantify the amplitude of movement) decreased from 40.5% with solid food to 38.3% under deprivation. His conclusion that “the total time spent by rats in some form of activity does not alter in food deprivation” (p. 317) is controverted by a study (145) in which direct observation revealed a 20–30% reduction in the “inactive” categories by the third day of deprivation. And it would certainly be premature to assume that a simple complementary relation holds between ingestive activity and wheel running, for Hamilton (94) has shown that the hungry rat which

spends considerable time drinking a saccharin solution exhibits the usual elevation of running scores.

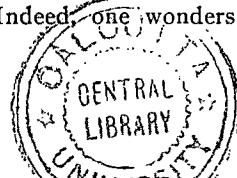
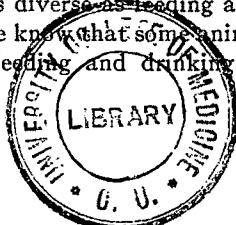
THE PROBLEM OF LOCALIZATION

For the most part, this review has steered clear of the extensive and complex literature dealing with electrical and chemical stimulation of the brain. We should mention one issue, however, which has important implications for our conception of the role of the brain, and in particular of the hypothalamus, in motivated behavior.

It is well known that stimulation of various hypothalamic areas can evoke behavior similar to that displayed in the presence of natural drive states, and that the kind of behavior evoked may depend on locus of stimulation. These findings, along with the ablation literature, have led to the conception of hypothalamic "centers" which "integrate" various inputs to induce states of drive. Valenstein and his colleagues (see 206, 208) have doubts, based on their findings that the kind of behavior elicited by hypothalamic stimulation can often be changed without alteration of the site or parameters of stimulation. For example, if an animal displaying stimulation-bound feeding is stimulated repeatedly in the absence of food, it will typically begin to display another form of behavior such as digging, drinking, gnawing, or carrying. This new behavior also comes to be stimulation-bound, and will subsequently occur as often as feeding does when the opportunity to engage in both activities is offered. These investigators present extensive evidence (206) that this plasticity is not an artifact of the simultaneous activation of different systems, each with fixed characteristics.

Whereas the kind of behavior evoked by a particular stimulation can be modified, the behaviors themselves have a surprising degree of specificity. For example, rats showing stimulation-bound eating of food pellets may not respond to the same diet in powdered form, even if thoroughly familiar with it as food. Or they may lap from a drinking spout but refuse to drink from a cup, even if the cup is familiar and has a preferred fluid in it.

On the basis of these and many other findings, Valenstein and his co-workers suggest that hypothalamic stimulation may activate the neural substrates not of "drives" but of fixed action patterns. Such stimulation may appear to have motivating properties, in that the animal will make arbitrary responses to obtain goal objects. For example, it will barpress for food. It may do so, however, not because of an artificial "hunger" state, but because the release of the fixed-action patterns involved in feeding is reinforcing. As to plasticity, the authors (208) suggest that the stimulation may excite "the substrate for a group of responses that in a given species are related to a common state" (p. 30)—i.e. the sort of state which Holst & St. Paul (112) have called "mood." This suggestion seems strained when applied to behaviors as diverse as feeding and copulation (cf. 206), but perhaps it is not; e.g. we know that some animals in sexual conflict will engage in "displacement" feeding and drinking (see 61). Indeed, one wonders



whether the observations of Valenstein and his associates may be related to the problem of displacement activities. So-called "psychogenic" or schedule-induced polydipsia has been related to displacement phenomena (72). Under appropriate conditions, would one see schedule-induced gnawing, digging, or carrying? Schedule-induced running has been reported (140, 177).

A similar issue of localization arises in a quite different context. Parallelizing the assumption of hypothalamic "centers" for various forms of motivated behavior is the notion, implicit or explicit in many treatments of brain mechanisms in motivation, of an anatomically defined "reward system," in which stimulation has reinforcing properties. Thus electrical stimulation in LH usually is rewarding (cf. 108). The magnitude of the effect varies with nutritional condition—if sated, a rat will work less hard for LH stimulation (e.g. 108)—but this effect might simply reflect changes in the responsiveness of the "rewarding" cells. Hoebel & Thompson (110), however, have shown that much more than that is involved. As their rats were made artificially obese, the decrease in reward value of LH stimulation was accompanied by an increase in its aversiveness: the rats would work harder to turn programmed LH stimulation off. Therefore, whether such a stimulus is predominantly "rewarding" or "punishing" can depend on the state of the animal as well as on the locus and parameters of stimulation.

One wonders how general this phenomenon is. How many other "rewarding" brain sites might be made aversive by manipulation of the organism if we knew what manipulations to make?

AFTERTHOUGHT

We have not, of course, told the full story on even the two major topics which we chose to survey. Whether by reason of limited competence or judgment of our subscribers' interests, we have no more than hinted at the vast physiological literature related to sex and ingestion. We were aware of this restriction as we wrote, but we must admit to some surprise as we now discover that, for example, we have referred to no more than one or two experiments which are cited also in the chapters on reproduction in the 1966-70 volumes of the *Annual Review of Physiology*. While this may be an unfair index of our parochialism (or the physiologists'), it does reflect a situation characterized by less than optimal communication. A degree of separatism is justifiable, if not absolutely essential, in *Reviews* as in university organization, but in the laboratory its convenience often turns into handicap. Surely there is no area more appropriate for the exploitation of the interdisciplinary approach than the investigation of basic drives.

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BEHAVIORAL GENETICS¹

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INTRODUCTION

It is difficult to specify precisely the steps involved in the complex merging of psychology and genetics that has resulted in the field of behavioral genetics. Perhaps Calvin Hall's chapter on psychogenetics, which appeared in Stevens' *Handbook of Experimental Psychology* in 1951 (151), was the first explicit sign of the origin of the field. Certainly the publication in 1960 of *Behavior Genetics*, by Fuller & Thompson (125), indicated a fully developed self-awareness of an important new specialty. The appearance of *Annual Review* chapters in 1960 (115) and 1966 (261) made clear that behavioral genetics was now viewed, even by those not working actively in the area, as an interdisciplinary field of general interest and significance.

Recent years have witnessed a variety of developments that attest to the continued vigor of activities in this area. Efforts are currently well along in the establishment of a behavioral genetics society which will have a broad and interdisciplinary membership. A journal entitled *Behavior Genetics* has also been initiated and will provide a unified outlet for animal and human behavioral genetics research. While at the beginning of this decade there were no formal graduate programs in behavioral genetics, there are now several such programs (University of Colorado, University of Illinois, University of Minnesota, University of Texas) and others are being initiated. A number of summer training institutes and conferences for graduate students and postgraduates have been conducted in recent years, as well as a variety of symposia and research conferences, e.g. Center for Advanced Study in the Behavioral Sciences (175), Rockefeller University (134), and Dorado Beach (338). Of special interest is the invited address to the annual American Psychological Association Convention by the distinguished geneticist Theodosius Dobzhansky (84).

Examination of the literature during the past 5 years reveals a sharp and unmistakable increase in the number of relevant publications. It also ap-

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pears that the research represented in these studies has been broadened significantly and now extends from the molecular to the societal. The most noticeable shifts in research orientation have involved a reduction in the number of simple comparisons of inbred strains of animals, and an increase in quantitative genetic analysis at both the animal and human levels, molecular and physiological studies, single gene analyses, and a growing interest in evolution.

Several major volumes and monographs appeared during the period covered by this review. Most of the research monographs will be referred to in the appropriate sections below. However, it may be useful to mention some publications of a more general nature at this point. While a few years ago teaching materials that could be used for undergraduate and graduate instruction in behavioral genetics were limited, there are now a number of appropriate volumes available: Parsons (307); Manosevitz, Lindzey & Thiessen (279); Hirsch (175); Spuhler (376); Glass (134); Vandenberg (410). The teacher faced with a classroom of students who have limited backgrounds in genetics should find Lerner's recent book (236) a useful means of giving students some familiarity with basic genetics. The instructor teaching introductory psychology who wishes to introduce his students to behavioral genetics may find the elementary discussion by Hirsch & Ksander (181) helpful.

Space limitations prohibit a comprehensive and detailed review of all the relevant literature during the past 5 years. We have attempted to comment upon those studies that are most significant, and at the same time we have tried to provide reasonable breadth of coverage. Several reviews of the literature in this area have been published (e.g. 35, 81, 91, 106, 137, 259, 260, 287, 412) and the reader may wish to consult them for additional information.

This chapter is arranged in five major sections: (a) pathways of gene expression; (b) sensory and cognitive abilities and learning; (c) personality, temperament, and social behavior; (d) evolution; and (e) research methodology.

PATHWAYS OF GENE EXPRESSION

Ultimately many or all gene effects on behavior will be linked to DNA structure, action, and processes of development. This section is devoted to reviewing three selected areas in behavioral genetics that promise early success in identifying such associations. The first part deals with audiogenic seizure susceptibility; the second with alcohol preference and aversion; and the third, with studies of the influence of external environment upon gene effects.

SEIZURE SUSCEPTIBILITY

Several species including domestic and *Peromyscus* mice, guinea pigs, rats, rabbits, and man are known to convulse occasionally when exposed to

a loud auditory stimulus (351). Without a doubt, the most systematic work bearing on the genetics and physiology of audiogenic seizure susceptibility has been done with *Mus musculus*. This section will review these findings. Other reviews with different emphases are available (126, 167).

Genetic evidence.—The transmission mechanisms for seizure proneness have never been clarified since the earlier suggestions that one, two, or several genes control audiogenic seizure susceptibility. The highly susceptible dilute color DBA and nonsusceptible and nondilute color C57 inbred lines have frequently been the object of investigation. The dilute locus (*dd*) in linkage group II has received special attention because of its apparent association with seizure susceptibility (188) and its relation to reduced phenylalanine hydroxylase activity (58), a predisposing condition for convulsions in man.

It is apparent that no single genetic model can incorporate all the data. Fuller & Sjursen (124) detected seizing in 11 mouse strains tested, including DBA/2J, LP/J, 129/J, RF/J, AKR/J, LG/J, SJL/J, CBA/J, SM/J, BALB/cJ, and C57BL/6J. The phenotypic patterns of susceptibility differ widely. For example, the first six strains mentioned are seizure susceptible beginning about 3 weeks of age, whereas the latter five first convulse around 4 weeks of age. It is unlikely that one particular genetic hypothesis would be valid for all strains. Moreover, Schlesinger, Elston & Boggan (350) point out that the genetic inference will vary for DBA/2J and C57BL/6J strains and their segregating groups depending on age and which aspect of the seizure is assessed (i.e. wild running, clonic or tonic seizures, or death). According to these investigators at least two separate genetic systems operate—one determining wild running and one affecting the tonic seizure. If a single locus is involved it cannot be the one associated with dilute coat color, as neither segregants between dilute and nondilute mice, nor inbred mice differing only at this locus, give the expected result (168, 350). Recently another allele, *asp*, located at a locus in linkage group VIII, has been implicated (60). This double recessive apparently accounts for much of the variance between DBA/2J and C57BL/6J mice. The genetic situation is complicated by the fact that activity differences among animals within a strain (DBA/1J) relate to death rate under auditory stress, and that nearly any inbred strain can be induced to seize with prior exposure to auditory stimuli during a critical age period. Strains are differentially affected by this treatment, again suggesting that the underlying genetics are complex.

Physiological evidence.—Research in the past 5 years has turned from the study of energy cycles, oxidative phosphorylating systems, and endocrine factors to the investigation of neurotransmitter substances and the peripheral processing of sensory data. Schlesinger and his associates have vigorously pursued the study of the neurotransmitter substances serotonin (5-HT), norepinephrine (NE), and gamma-aminobutyric acid (GABA)

(351). Using a variety of genetic and pharmacologic techniques, 5-HT, NE, and GABA levels in the brain have been elevated or decreased in an attempt to associate seizure susceptibility to these transmitters. In summary, these investigators find that decreases in 5-HT and NE, either singly or in combination, elevate seizure susceptibility in both genotypes, whereas an increase in these substances and GABA decreases seizure susceptibility. Moreover, the manipulation of these brain substances also effects metrazol-induced and electroconvulsive-induced seizures in a similar fashion, leading to the suggestion that seizure susceptibility is merely another index of brain excitation. Similar hypotheses were held in earlier years.

The second major strategy for investigating seizure proneness has been to subject mice to a "priming" auditory stimulus either before audiogenic seizures can normally be elicited or when the mice are anesthetized and cannot seize. Apparently any mouse strain subjected to auditory priming will seize at a later time when exposed to the same noise, including C57BL/6J, BALB/cJ, SJL/J, and various crosses between these strains (170). DBA/2J is apparently at such a high convulsive level that priming does not enhance susceptibility. In crosses between DBA/2J and C57BL/6J strains, primed F_1 offspring deviate in the direction of the C57BL/6J. Priming is ineffective before the ears become functional around 14 days of age and is maximally effective between the ages of 16 and 19 days (169). The priming effect is apparently long-term and can be restricted to one ear by temporarily blocking hearing in the other during the priming interval (123, 169). The ipsilateral effect and the fact that anesthetics that dampen the CNS have no inhibiting effect on priming strongly implicate peripheral mechanisms, possibly in the ear itself. The generality of the findings point to a fundamental mechanism of control. Peripheral mechanisms may not only regulate the onset of seizing but also its inhibition, inasmuch as Ralls (323) demonstrated that DBA/2J and BALB/cJ mice lose much of their auditory sensitivity with age.

ALCOHOL PREFERENCE AND AVERSION

There have been several recent studies of the genetic and physiological mechanisms underlying alcohol preference and aversion in mice, rats, and man (100, 238, 259, 331, 349). Work has focused on the nature of the genetic system and metabolic variations that influence alcohol ingestion.

Genetic evidence.—Both rats (99, 100) and mice (331, 349) respond to selection pressure with increases and decreases in alcohol preference. Selection is effective over a number of generations, and heritabilities are low to moderate. Studies of this kind continue to support the notion that alcohol ingestion is polygenetically controlled. Fuller (118) points out that mouse strain differences are partly attributable to the drinking schedule imposed and the number of choice solutions available during the test.

Sex differences are in some cases prominent in mouse and rat strains.

Eriksson & Pikkarainen (101), for example, point out that female C57BL, but not CBA female mice, drink more than their male counterparts. Strain differences in males account for about 69% of the variance, and in females about 94% of the total variance. Similar differences have been found for rats selected for high and low preference (100).

Other evidence indicates that Maudsley Reactive (MR) and Nonreactive (MNR) rat strains selected for emotional behavior in an open field also differ in their preference for a 5% ethanol solution (32). The MNR strain displayed a higher preference score. A 2×2 diallel cross gave evidence for an additive genetic system with high heritability (about 70%). Interestingly, Brewster reanalyzed earlier data collected by McClearn & Rodgers (262) on the C57BL/Crgl and A/Crgl mice, and data by Fuller (116) on C57BL/6J, C3HeB/FeJ, A/J, DBA/2J, and F₁ groups, and found high estimates of additive variance (82% and 86% respectively) and evidence for incomplete dominance. In another study (95) "domestic hooded rats" and "wild pack rats" (genus *Neotoma*) were found to differ in the amount of absolute alcohol consumed under forced and choice conditions.

Perhaps the most interesting recent genetic experiment is that by Fuller & Collins (122). Using C57BL/6J high-preferring mice and DBA/2J low-preferring mice, nonparametric analyses with F₁, F₂, and backcross generations revealed that for both total ethanol consumed and amounts relative to water, a two-unit model closely fits the data. This result contrasts with those discussed above but may indicate that a major portion of the phenotypic variance in some strains is due to major gene effects. In view of recent biochemical findings (see below) this observation may be highly significant.

Physiological evidence.—Early evidence suggested that mouse strain differences in alcohol preference corresponded to parallel differences in the liver enzyme alcohol dehydrogenase (ADH), with the C57 genotypes scoring higher on both variables than DBA and other strains (331). ADH activity is the first step in the metabolism of ethanol to acetaldehyde and therefore could be of critical importance. Although individual reports vary in their conclusion about metabolic differences between the C57 and other genotypes (206, 331, 349, 352), it is generally agreed that ADH activity and voluntary consumption of ethanol are at least partially related and that strains do not drink beyond their metabolic capacity (333). When, however, ethanol ingestion is high over long periods and coupled with lowered nutrition, striking pathologies appear in some strains (e.g. C3H/HeJ and C57BL/10J) that correspond to those often found among human alcoholics (333). Obvious intoxication can be produced in C57BL/6J animals by restricting their intake to a short period of the day (118). Mice tend to compensate for fluid deprivation by lowering their preference for this dipsogenic drug (387). C57BL/Crgl mice, but not RIII mice, return toward their original preference when fluid balance is re-established by an *ad libitum* choice schedule or by injection of isotonic saline.

Conversely, alcohol ingestion is increased severalfold when C57BL/Crgl females are nursing young (390). This elevation is related to increases in liver size, metabolic capacity to dispose of blood alcohol, and the number of mice being lactated (391). Population density or other stresses, on the other hand, tend to decrease, not increase, alcohol ingestion (331, 332, 389), which casts some doubt on the common assumption that alcoholism and anxiety are positively related. In mice, at least, metabolic considerations predominate.

While previous research has concentrated on ADH as the rate-limiting enzyme, investigation is now turning to the enzyme responsible for the oxidation of acetaldehyde, namely aldehyde dehydrogenase (ACDH). ADH and ACDH enzymes in combination may account for the major restrictive pathway of ethanol breakdown through the citric acid cycle (349, 352, 362). ACDH may assume the primary importance, as drinking and nondrinking strains of mice differ by 300% in this enzyme (362).

In any case, it is interesting to speculate about the relation between the two crucial enzymes discussed and the two genetic units suggested by biometric analysis of the behavioral variance (122).

GENE REGULATION

Recent developments in microbiology and developmental genetics offer behavior genetics new challenges. Not only must behavior geneticists concern themselves with transmission of genetic information but also with regulators of gene expression. Perhaps not more than 10% to 20% of the assembly of genes are active at any one time, depending upon the tissue and environment considered (21). Attention, therefore, is turning to models of gene regulation that may account for some of the dynamic qualities of behavior (33, 286).

The regulatory mechanism best known involves the molting patterns of *Diptera* and midge (*Chironomus*) from larva through pupa and adulthood. The sequence depends upon the molting hormone, ecdysone, acting on loci of chromosomes I and IV (21, 57). Hormones in mammals which appear to stimulate messenger RNA production and protein synthesis probably underlie many important behavior processes (382).

Not many clear examples of gene induction and behavior are yet known. Environmental stimuli such as avoidance training can alter the kinds and amounts of RNA and protein formed in the brain (127, 424). Hydén (192), in particular, details how environmental influences can alter primary genetic action. In one experiment "right-handed" rats were trained to use their non-preferred paw to obtain food. There were significant increases in RNA in the motor cortex associated with the change in preference, and the bases used to form RNA (adenine, guanine, cytosine, and uracil) showed unique changes. The (G+C)/(A+U) ratio decreased from 1.72 to 1.51. Moreover, the nuclear RNA formed was said to have a DNA-like base ratio composition,

Apparently genes in neural centers appropriate to the behavior involved are differentially activated and expressed as messenger RNA.

Studies using other techniques can be interpreted in a similar fashion. Drugs that selectively interfere with DNA transcription, RNA translation, or protein synthesis are known to disrupt learning, memory, and reproductive functions (18, 110, 298, 392). Similarly, amino acid substitutions in the adrenocorticotrophic hormone (ACTH) can limit extinction of an avoidance response in rats (28). It is just such gene regulated changes that would be expected to influence behavior and evolutionary processes.

Biochemical brain changes related to enriched or deprived environmental and social settings (especially those involving enzyme effects) strongly suggest a regulatory function at the level of DNA (11, 69, 340). In fact, the immense increase in the amount of DNA material in higher species without substantial change in the number of biochemicals present may be interpreted as evidence that such gene-level regulatory processes are of considerable importance in the process of evolution (33).

SENSORY AND COGNITIVE ABILITIES AND LEARNING

In this section we will review studies which focus primarily on the genetic basis of behavioral capacities, ranging from single-gene studies of sensory functions in inbred mouse strains to general intelligence in man.

SINGLE-GENE EFFECTS ON SENSORY AND MOTOR FUNCTIONS

Locomotor coordination.—Behavioral observations on neurological mutant mouse strains have provided a useful tool for investigating locomotor behavior. Fox (111), using a battery of reflexological and behavioral tests, has been able to classify mutants as static, in which vestibulocerebellar lesions can be detected early in the neonatal period (*twirler* and *waltzer*), and progressive, in which the defect begins later and the condition of the mice deteriorates (*ducky*, *dystonia musculorum*, *reeler*, *jumpy*, and *jittery*). Thiessen (384) has studied several developmental features of the *wobbler-lethal* mouse, concluding that behavioral observations can sometimes be used to identify the mutant before brain lesions (myelin degeneration) are obvious. Sidman et al (368) have developed a sensitive conditioned suppression method for studying auditory thresholds and have applied their technique to the study of *pirouette*, *quaking*, and *reeler* mutants. *Pirouette* mice have an elevated auditory threshold which accompanies a loss of outer hair cells in the cochlea. Lastly, in a careful series of studies, Erway, Hurley & Fraser (105) have demonstrated that a swimming disability of *pallid* can be rectified by supplements of manganese during prenatal development and is related to the re-establishment of otoliths in the vestibular portion of the inner ear. A phenocopy can be produced by a dietary deficiency of manganese. This is one of the rare cases in which the phenocopy-inducing agent has a reciprocal effect on the mutant itself.

Retinal degeneration.—In most C3H inbred stocks, as well as several other commonly used strains, photoreceptor cells degenerate by the second postnatal week (217, 366), resulting in a 10^{-5} reduction in visual sensitivity, although the spectral sensitivity curve is unaltered (365). Unfortunately, the C3H line has been used in visual experiments by investigators unaware of the magnitude of the defect; it appears likely that the major learning disabilities found can be attributed to this mutant gene (30, 31). It is probable that the peripheral lesion is followed by degeneration of higher visual centers (150). Several lessons can be gained from a study of selected neurological mutants, not the least of which is that subtle aspects of behavior may be understood better once major or gross relationships are identified.

Albinism.—Albinism results from an inherited loss of melanin formation in skin, hair, and eyes. Oculocutaneous albinism in mice, and probably other species including man, is apparently due to the lack of tyrosinase synthesis and hence a failure of tyrosine to be converted to dopa, dopa-quinone, and melanin (433). At the *C* locus in mice, the allelic series, *agouti*, *chinchilla*, *himalayan*, *extreme dilution*, and *albino*, regulate the number and size of melanosomes and the amount of melanin deposited on each. Tyrosinase activity is highest in *agouti* and totally absent in *albino* (433). Several reviews of the genetic and physiological control of coat color in mammals are available (79, 322, 388).

Albinism in man has been known for over a century to result in photophobia (109), and since the early studies of Keeler & King (219) it has been suggested that albinism and behavior are related. Mice, in particular, have become favorite subjects for investigation, because much is known about coat color variations in this species and mice offer possibilities for precise genetic control (433).

Several experiments with mice and rats suggest that some strain differences in behavior may be related to albinism (70, 83, 239, 240, 363). Much of this research was stimulated by the finding of Winston & Lindzey (430) that albino inbred strains and albino mice derived from F_2 and backcross generations were deficient in water escape performance. The increased latency to escape water has been amply verified (119, 388, 420), and it is generally agreed that albinism is associated with reduced activity under bright, white-light conditions (75, 119, 171, 388). Importantly, such genetic differences in activity often disappear under red or dim light (75, 119, 388), implicating photophobia as the primary mechanism of control. In strong confirmation, DeFries (73) selected for high and low activity from an F_3 population containing albino segregants and found that he was concomitantly selecting for high and low frequency of albinism.

Photophobia cannot account for all differences between albino and pigmented mice, as many behavioral variations can be found in situations where vision is relatively unimportant. For example, albino F_2 mice are inefficient in active avoidance learning but highly efficient in passive avoid-

ance learning (431); they also appear to be less sensitive in their geotaxic response to gravity (388), and respond less readily to an audiogenic seizure stimulus (145). In other segregating populations albino mice show a fear of heights (3) and a slight advantage in sexual competition (242). Albinism even leads to a reduction in alcohol preference in a two-bottle water-alcohol preference situation (171). Clearly the albino effect is extensive and cannot always be related to the absence of visual pigment. It would be interesting to examine all the alleles at this locus in an attempt to find a relation between tyrosinase activity and some of the behaviors implicated in the above studies.

Screening on multiple measures.—A number of investigators have examined inbred mouse strains on batteries of different behavioral measures. While for convenience these studies are considered here, many of the individual measures could properly be discussed with studies of motivation and temperament later in this review. Abeelen (1) has studied a number of different behaviors in several groups of mice that differ at single loci. It is Abeelen's hope to construct an "ethogram" of natural behavior by observing natural units of behavior. Approximately 25 species specific behaviors were recorded for the following alleles: *maltese dilution*, *pink-eyed dilution*, *loop-tail*, *jerky*, and *waltzer*. The latter three are neurological mutants. Only the pink-eyed gene among the non-neurological mice had obvious effects on behavior, among which was a reduction in reconnoitering, a decrease in lifting of one forepaw and tail rattling, and an increase in gnawing at a wall. Unfortunately the number of animals tested was often small and the background genotypes were not standardized, so it is difficult to interpret these findings.

Thiessen, Owen & Whitsett (388) have screened 14 genes segregating on the C57 background for open-field activity, negative geotaxis, water-escape latency, and wheel revolutions. As with many other studies, these loci are related to changes in coat color. The genes studied were *black and tan*, *tanoid*, *intermediate agouti*, *yellow*, *viable yellow*, *brown*, *buff*, *himalayan*, *caracul-J*, *misty*, *white*, *steel-Dickie*, *tortoise shell*, and *dominant spotting*. Both dominant and recessive genes were included in this survey, and several chromosome units were represented. Some gene specificity was evident, which gave the investigators hope that "behavioral genes" could be mapped on chromosomes.

Werboff, Anderson & Ross (420) studied brown, black, and albino littermates obtained from a four-way cross between inbred mouse strains. The animals were tested for open-field activity, visual and spatial learning in a water maze, submersion time in water, and amphetamine effects on submersion time and water escape. Albino mice with weights attached to their tail submerged less readily than black mice, and were poorer learners on visual and spatial problems. The results could indicate a pleiotropic effect on several homeostatic mechanisms.

NORMAL HUMAN ABILITIES

The broad spectrum of human abilities which have received attention from the behavior geneticist during the period under review ranges from general intelligence to such simple sensory capacities as color vision and taste sensitivity. There have been a number of excellent reviews and discussions of this research (180, 190, 198-200, 378, 395, 406, 408, 409).

General intelligence.—Perhaps the most important paper on this topic during the period under review is that of Burt (48), who presents data on the resemblance in IQ of 53 pairs of identical twins reared apart, as well as summarizing his own work and that of others on the correlation in IQ of persons of differing degrees of relationship. Not only is his group of separated pairs the largest so far reported, but all twins were separated within the first 6 months of life, and there was no selection for environmental similarity—in fact, there was a slight negative correlation between the socioeconomic status of homes in which separated twins were reared. This appears to have resulted from the scientifically fortunate circumstance that in many of his cases one twin was given up for adoption by impoverished parents unable to care for both, the one twin thus being reared in a relatively well-to-do environment, while the other remained in a lower-class home. The Stanford-Binet IQ's of the separated twins correlated .86. This is not very much lower than the figure of .92 obtained by Burt for 95 identical twin pairs who were reared together. In reply to critics (244, 381), Burt (49) reports that for the subgroup of 27 pairs separated within the first month, the IQ correlation of .84 is essentially as high as for the entire group. [Along the same lines, Vandenberg & Johnson (414) reanalyzed data from separated identical twins in studies prior to Burt's, to show that twins who are separated relatively late do not turn out to be more alike in IQ than those separated relatively early.] Prenatal environmental effects are not ruled out as contributing something to the resemblance of the separated pairs in Burt's study, but if one places very much weight on prenatal environment as a cause of similarity, it creates difficulties elsewhere—for example, why should parent and child be almost as similar in IQ as siblings, and why are not fraternal twins more alike than they are? As for the cultural environment, Burt does not exclude its effects altogether; in fact, he presents evidence of a modest correlation of .26 between rated differences in cultural conditions and IQ differences for the separated monozygotic pairs. (Note that such a correlation says nothing about the relative importance of heredity and environment: it merely indicates that the MZ IQ differences that *do* exist—which are necessarily environmental—show some association with ratings of home conditions.)

A number of studies during the period present data further supporting the well-established generalization that identical twin pairs tend to be more similar on measures of general ability than same-sex fraternal twin pairs.

Huntley (190), with a composite vocabulary test given to 85 identical and 135 fraternal pairs, obtained correlations of .83 and .66, respectively; Nichols (300), with the National Merit Scholarship Qualifying Text (NMSQT), obtained correlations of .87 and .62 for 315 and 209 male pairs, and correlations of .88 and .65 for 372 and 273 female pairs. Schoenfeldt (354), for a general intellectual factor from the Project TALENT battery, obtained correlations of .78 and .39 for 150 and 53 male pairs, and .81 and .52 for 187 and 103 female pairs. Obviously there is some study-to-study fluctuation in these correlations, which could result from differences in the measuring instruments, differences in the populations, differences in the accuracy of zygosity determination, and sampling error. As discussed in a subsequent section of this review, estimating heritability from twin data is a somewhat tricky business, but on typical assumptions these twin correlations suggest heritabilities a little lower than the .86 "broad sense" heritability suggested directly by Burt's data—perhaps more on the order of .75 (the authors give various estimates ranging from .60 to .88). Since any heritability estimate is specific to a population and a measure, some variation is to be expected.

Heritability coefficients do not provide much information about the nature of the genetic determinants of IQ. One potential source of such information lies in studies of the effects of inbreeding. If a substantial proportion of the genes involved show dominance, then the mating of related persons will increase the probability of matching up recessive alleles. If the direction of dominance is predominantly toward high intelligence, the trait will tend to show "inbreeding depression" among the offspring of related marriages. Schull & Neel (355), who were carrying out large scale studies of the genetic effects of the atomic bombings in Japan, took advantage of the relatively high frequency of cousin marriages in that country to give a Japanese version of the Wechsler Intelligence Scale for Children (WISC) to 2111 Hiroshima school children, half of them the product of marriages of related individuals (in the range of first to second cousins) and half whose parents were unrelated. After controlling statistically for socioeconomic differences between the groups, they found an average depression of 3-5% in the offspring of first cousins, slightly greater for verbal than for performance IQ. Thus there is some evidence for directional dominance of genes influencing intelligence within the normal range. It is also interesting to note that IQ proved to be one of the most sensitive indicators of inbreeding depression in their studies, which also included a variety of other psychometric and anthropometric measures.

Special abilities.—Another way of trying to understand the genetic and environmental influences on general intelligence is to break it down into more specialized component abilities. A good deal of effort has been expended along these lines in the period under review. Nichols (300) reports an analysis of the subtests of the NMSQT after partialling out statistically the large general factor. While the results were rather erratic, there was some indica-

tion that the specific abilities were of about the same level of heritability as the general factor. There was no consistency across the sexes as to the relative heritability of the specific abilities. For the Project TALENT twin sample, Schoenfeldt (354) reports data on three differential aptitude factors: visual reasoning, perceptual speed, and memory. He found somewhat lower heritabilities for these than for his general intelligence factor, with the median coefficient in the .50s; again there was no consistency across the sexes. Vandenberg and various associates have been especially active in this field, examining the performance of high school age twins from the Louisville area on the Wechsler Adult Intelligence Scale (WAIS) (26), the Differential Aptitude Tests (DAT) (27, 409), the Primary Mental Abilities (PMA) (255, 408), a battery of spatial visualization tests (411), Guilford's divergent thinking tests (416), and a variety of other measures. Some of the same tests have also been given to a small Atlanta twin sample by Osborne & Greger (305, 306). The results of all these studies may be summarized by saying that identical pairs are typically more similar than fraternal pairs on cognitive ability traits, and that there are no very clear indications that the similarity is consistently greater for some abilities than for others, when measured with equal reliability. The lack of agreement across the sexes in the Nichols and Schoenfeldt studies is perhaps best treated as a particular case of this general phenomenon, which surely constitutes a challenge for the theorist. We will encounter it again when we consider personality.

Several studies with older (197, 311) and younger twins (113, 416) suggest that rather similar conditions prevail across the life span.

In addition to analyzing the variance of single traits into genetic and environmental components, one can analyze the covariance between traits in much the same way; it is often instructive to ask whether what is shared by two correlated traits is primarily common genes or a common set of environmental influences. Furthermore, one can readily extend this logic to a factor analysis or other multivariate analysis of a whole matrix of covariances (or correlations). Analyses along these lines have been reported for the PMA tests (255, 405), the WISC (408), the DAT (27), and a Finnish test battery (311). In some analyses, a large general factor (Spearman's "g"?) emerged in the genetic covariance; however, this was not the case in others. Differences in method of analysis may have been partially responsible—but in any case, for dependable results these approaches probably require somewhat larger sample sizes than have been used so far.

Sensory capacities.—Active work continues with color vision and the ability to taste low concentrations of phenylthiocarbamide (PTC). No attempt will be made here to provide a serious review of this extensive literature; an illustrative example or two must suffice.

A good deal of recent effort has been directed toward mapping the frequency of color and taste insensitivities in various human populations, among which considerable variation exists. Interest in this area has been

stimulated by the suggestion of Post (320) that the relatively high incidence of red:green color blindness in long-civilized peoples may represent a relaxation of selection against such defects in these populations. Evidence indicating a low incidence of color-vision defect in primitive peoples continues to be reported (e.g. 90, 326), but there are some discrepancies, and some strongly skeptical views persist (4).

Research on sensitivity to the taste of PTC and other thiourea derivatives is being carried out on various fronts. A particularly extensive research program is that of Kaplan and his associates (summarized in 208), using 6-*n*-propylthiouracil (PROP), a substance which has technical advantages over PTC but is otherwise similar. Taste thresholds for another unrelated bitter substance (quinine) and a sour substance (hydrochloric acid) were measured in the same subjects. A twin-sibling study (210) showed the threshold for tasting PROP, but not for the other substances, to be under a high degree of genetic control. Many variables affecting the taste threshold for PROP seem not to be closely related to the single taster gene. For example, the thresholds for the two bitter substances PROP and quinine were correlated across individuals and tended to behave similarly with respect to age and smoking, the menstrual cycle, and food preferences. An interesting exception was provided by duodenal ulcer patients, who were relatively sensitive tasters of PROP, but not of quinine or hydrochloric acid (in comparison to gastric ulcer patients or normal controls).

MENTAL DEFICIENCY

Two categories of mental retardation are often distinguished: severe retardation, with IQ less than 50, and mild retardation, with IQ from 50 to 70 (or whatever is specified as the lower limit of normal intelligence). Major chromosomal anomalies or single gene defects tend to produce severe retardation; most mild retardation is taken as merely the low end of the normal distribution of intelligence, whose genetic basis is presumably polygenic (for discussions, see 200, 438). After glancing at some studies of familial incidence of mental retardation, the present review will focus on the more severe forms, concerning which there is more genetic information. But even here the treatment will necessarily be brief. Fuller reviews and discussions are available (e.g. 20, 439) and other references are cited below.

The massive study of Reed & Reed (325) began with the grandparents of 289 Minnesota mental defectives initially studied between 1911 and 1918. More than 82,000 of the descendants of these grandparents were traced, some over as many as seven generations. The bulk of the Reeds' report consists of detailed pedigree charts of these kindreds, including a large number of persons for whom IQ scores were available through schools and other agencies. Summary data on the frequency of retardation among the offspring of consanguineous unions and various types of marriages of normals and retardates are presented and discussed. There is also information on assortative mating, sibling and twin differences, and the relation between IQ

and family size (slightly positive, when childless individuals are properly taken into account).

Another study of fertility and mental deficiency is reported by Åkesson (7), based on 7533 people living in a random sample of rural parishes in southern Sweden. He found 88 mild and 44 severe mental defectives in this population, or 1.8%. The mildly retarded produced decidedly more offspring than a comparison group of normals. However, if the severely retarded group (who were "practically infertile") are added, the retarded appear to be only slightly overproducing. In another population study, this time in west Sweden, Åkesson (8) found 105 cases of severe mental deficiency among 17,303 inhabitants, or .6%, agreeing well with the previous study. About 20% could be identified as to probable cause (with trisomy-21 the most common); the remaining 80% were of unknown etiology.

Chromosomal anomalies.—After the dramatic discoveries of the preceding 5 years, the last 5 have been relatively quiet. Still, a large literature is developing in this area, which we cannot hope to survey in detail. Fortunately, there are available a number of excellent books, reviews, and bibliographies (51, 63, 209, 312, 380, 401, 436). The major trisomies of autosomal chromosomes 13, 18, and 21 appear to be firmly established, as well as partial deletions of the short arm of chromosome 5 ("*cri du chat*" syndrome), and of the short and long arms of chromosome 18. All produce severe mental defect, among other symptoms. Those that survive presumably represent relatively benign conditions. Some 20% of spontaneously aborted fetuses show chromosomal anomalies (51, 404), and the more gross aberrations may not even get to that stage. Many new syndromes are being reported, some with quite complicated chromosomal aberrations postulated, involving several breaks and insertions (e.g. 76). Some of these anomalies are balanced (i.e. merely involve a rearrangement of normal genetic material) and seem to have no obvious phenotypic effects, although they can lead to unbalanced conditions in offspring. Others are unbalanced (i.e. involve a duplication or deletion of genetic material) and do more or less severe damage, almost invariably including mental defect. Lejeune (234) has suggested that an excess and a deficiency of particular chromosomal material may tend to have opposite somatic effects (e.g. a partial monosomy of chromosome 21 led to hypertony, a prominent nose, hypocanthus, a narrow pelvic angle, and other "antimongol" symptoms); mental retardation, however, shows up in either case.

Anomalies of the sex chromosomes have less severe effects on intelligence: an extra X or Y is compatible with normal intelligence or mild retardation. However, a multiple duplication like XXXY or XXXX (or more) usually leads to serious mental deficiency (234). For one ability, the capacity for visualizing spatial relationships, there has been discussion of a simple mode of sex-linked inheritance. Garron (129) provides an up-to-date review of the evidence on this topic.

Mosaicisms (i.e. mixtures of two or more cell lines with different chromosome complements) are increasingly often reported, particularly among the sex chromosomes, but for autosomes too. Such mosaicisms may vastly complicate interpretations, since the cell proportions may be quite different in different organs, and may change over time. But by the same token, transient mosaicisms should sometimes be capable of providing tests of hypotheses about critical developmental periods.

Some chromosomal anomalies are inherited (via translocations), but the majority are sporadic, of unknown origin. An external environmental agent (radiation? viruses?) is often suspected (71, 330, 402). Some such theories would predict an association between different chromosomal anomalies. While associations of this kind have been reported (401), there is also negative evidence (160).

Single-gene defects.—Among the 1545 syndromes known or suspected to be due to a single gene which are described in the second edition of McKusick's *Mendelian Inheritance in Man* (268), 134 have mental retardation listed as one of their symptoms. Seven of these syndromes involve autosomal dominant genes, 112 involve autosomal recessives, and 16 are X-linked. In total, McKusick's book lists 793 autosomal dominant, 629 autosomal recessive, and 123 X-linked syndromes. Thus those single-gene syndromes which include mental retardation constitute only about 1% of the dominant category, while they account for 18% and 13% of the other two. No doubt the shortage of dominant syndromes is in part due to the difficulty of recognizing such syndromes in disorders which effectively prevent the reproduction of affected individuals, such as severe congenital mental defect. But it is conceivable that the mode of gene action is also involved: according to McKusick, recessive genes typically act on enzyme systems, dominant genes on structural protein—and this also might have some relevance to theories of intellectual development.

Morton and his colleagues (82, 297) have attempted to estimate the role of major genes in the etiology of severe mental defect. After excluding cases due to known chromosomal anomalies, trauma, infection, and the like, they examine "high-risk" families, with more than one severe defective in a sibship. From the relative frequency of such sibships, and the degree of parental consanguinity, they arrive at estimates of the number of gene loci involved. Based on a Wisconsin population which involved 49 high-risk families, and Penrose's earlier Colchester data, they conclude: that on the order of 114 recessive loci contribute to severe mental defect; that these are maintained by mutation, not heterozygote advantage; and that they account for only about 12% of severe mental defect. The remaining 88% of sporadic cases must thus be due to other causes, perhaps including unrecognized exogenous factors, small chromosomal aberrations, and the heterozygous expression of usually recessive genes.

Space does not permit a systematic review of the literature on individual

single-gene defects producing mental retardation. Some examples may be found described and discussed in (14, 187, 293). And readers wishing to curl up with the literature on gangliosidosis, the Prader-Willi syndrome, or the oasthouse urine disease can get a start in McKusick (268).

ANIMAL LEARNING

Maze learning.—Behavioral genetic studies of maze learning have used rats, mice, and dogs. Most of the studies have focused on strain differences and little work going beyond this has been reported (96, 155, 263, 314). Studies with mice have shown strain differences for T-maze reversal learning in four inbred strains, but no differences in performance as a function of intertrial interval (53). Heritabilities based on the four inbred strains ranged from .59-.70 for the various intertrial intervals. A study by Werboff & Anderson (419) on water maze learning evaluated the preference and use of visual or spatial cues in two inbred strains, A/J, C57BL/6J. Both strains of mice showed spatial preference but this was stronger in the pigmented C57BL/6J strain.

Avoidance conditioning.—Of all learning studies, avoidance conditioning studies have been the most popular. Several studies to determine optimal parameters for avoidance conditioning in mice have been reported. Royce (342) found that 3-sec CS-US intervals, 120-sec intertrial interval, and 400 V levels were optimal for avoidance conditioning, but that considerable variation exists around these parameters. There was also some limited evidence that CS-US interval may interact with genotype.

Several studies have demonstrated strain differences in active and passive avoidance conditioning (31, 52, 356, 425, 431). Bovet and his colleagues have reported a series of studies on genetic and environmental determinants of learning and memory. On the basis of studies using massed and distributed practice, extended training sessions with variable rest periods, one-trial avoidance learning, ECS, and various drugs, they suggest that a two-stage concept of memory storage, with the C3H/He as a "short term memory" strain and the DBA/2J as a "long term memory" strain, appeared to be a plausible interpretation. As discussed in a preceding section, the interpretation of such experiments is complicated by sensory defects in some of the strains involved. Research by this group using light or tone as the CS in avoidance conditioning studies indicated that strains may respond differently to various CS (303). Thus studies using various CS and genotypes would be useful. Data (from the DBA/2J inbred strain only) on active avoidance learning (jumping response) and passive avoidance, as reported by Wimer et al (425), using massed and distributed practice and immediate post-trial etherization, are contradictory to those reported by Bovet et al (31). The nature of the conditioned avoidance response (CAR) and other procedural differences may be responsible for the inconsistency. One might

expect that considerable work in the next few years will be devoted to reducing these differences. In a study (353) using seven inbred mouse strains and four F_1 hybrids (males only), in which the CAR was jumping onto a shelf, strain differences and heterosis were demonstrated. In some of these studies the same inbred strains of mice were tested (31, 353). Although there are differences in procedures and the CAR being measured, there is some agreement as to which strains are fast and which are slow learners. Roberts' work (327) on the effects of electric shock on skin resistance (presumably a measure of arousal) in inbred mice showed significant strain differences in skin resistance. On the basis of his results, it appears that there are genetic differences in aversiveness of grid shock and that strain differences in avoidance conditioning could be partly a result of differences in shock aversiveness. In a study (431) in which subjects were given the option of avoiding actively or passively, it was shown that albino mice do not differ from non-albinos in avoidance conditioning. However, albinos differ in the response mode of avoiding aversive stimulation; they usually avoid passively whereas non-albinos may avoid actively or passively.

Further evidence on the importance of genetic factors in CAR has come from selection experiments. Bovet et al (31) reported progress after only three generations of selection for higher avoidance scores, starting with a heterogeneous SWR mouse base population. They do not report the method of selecting breeding pairs or the criterion by which high CAR subjects were identified. A selection study reported by Bignami (23) used a random bred foundation stock of Wistar rats. Selection progress was made in both high-avoidance (RHA) and low-avoidance (RLA) lines by the fifth generation. An experiment by Broadhurst & Bignami (37) tested the two lines for CAR and in the open field test of emotionality, to determine if avoidance conditioning was associated with open-field reactivity. In the Maudsley lines the reactive strain had been shown to be low in CAR, and the nonreactive strain to be high in CAR. These results could have been due to fortuitous joint selection or to pleiotropy. The association between avoidance learning and open-field emotionality was not found with the Bignami strains. Thus, the two selection experiments appear to have selected for different and non-correlated characteristics. A subsequent factor analytic study (182) using the RHA and RLA lines from the eighth generation showed that at least one difference between the two lines is in activity.

PERSONALITY, TEMPERAMENT, AND SOCIAL BEHAVIOR

In this section we will review studies which focus primarily on genetic factors that influence temperamental and motivational aspects of behavior and social interaction. The range is from insects to man, and from normal behavior to pathological reaction. The reader is also referred to the earlier section "Pathways of Gene Expression" for relevant material on audiogenic seizures and alcohol preference.

ANIMAL STUDIES OF TEMPERAMENT, MOTIVATION, AND SOCIAL BEHAVIOR

Emotionality.—Behavioral genetic analyses of emotional behavior have typically been concerned with open-field activity and defecation in mice and rats. Although other dependent measures of emotionality have been used by comparative psychologists, such measures have not been used extensively in behavioral genetic analyses. Strain differences in open-field behavior of mice and rats have been reported in previous *Annual Reviews* (115, 261) and additional reports of strain differences have appeared in the last few years (e.g. 5, 78, 374, 423).

A genetic analysis of repeated open-field tests using F_2 and F_3 mice (161) showed that h^2 decreased on Day 2 for both defecation and activity, but the decrease in h^2 of open-field activity was due to a decrease in V_A and an increase in $V_E + V_{NA}$, while the decrease in the h^2 estimate for defecation was largely due to a decrease in V_A . Genetic correlations of Day 1 and Day 2 activity and defecation measures showed that activity and defecation on Day 2 are influenced by some of the same genes, but defecation on Day 1 appeared to be associated with different genes.

Results from five generations of artificial selection for high and low open-field activity using mice were reported by DeFries & Hegmann (74). Differences in defecation were associated with the significant differences obtained for activity. The results of their selection study were similar to those obtained in their preliminary genetic analysis based upon P_1 , P_2 , F_1 , F_2 , B_1 , B_2 , and F_3 generations. This study makes a valuable contribution by comparing various methods of estimating genetic parameters.

Bruell's (43) diallel study of the mode of inheritance of defecation in a maze revealed that in both inbred and hybrid subjects males defecated more than females, and that inbred females defecated more than hybrid females, but that hybrid males defecated more than inbred males. Thus, the mode of inheritance was heterotic but in different directions for males and females. Exploratory activity in the maze was positively correlated with defecation in males but negatively for females. It was suggested that these results are not as puzzling as they may appear at first. The observed sex differences are consistent with an interpretation of defecation as a territory marking response.

Broadhurst (36) summarized his previously reported diallel study of open-field defecation and activity using covariance-variance diagrams to present his results. Estimates of genetic parameters obtained through various techniques were compared, and genotype-environment interactions in developmental stability and change were investigated. He reported that for defecation heritability in the narrow sense decreased across the four daily trials. A finding similar to that of Hegmann & DeFries (161) was reported and in both studies it was suggested that this is a result of decreased additive variance.

Henderson (164) also performed a diallel analysis of open-field defecation

and activity using four inbred mouse strains. In addition he used three levels of pretesting stimulation. He found that pretesting experiences influenced the overall mode of inheritance of open-field defecation (but not activity), and that the pretesting treatments interacted with genotype. He suggested that inferences about mode of inheritance should be made with caution until a variety of pretesting experiences have been studied.

A comprehensive review of the literature on perinatal factors that influence emotionality showed that many pre- and postnatal factors affect emotionality and interact with genotype (203). This review should be consulted for a discussion of the methodological problems and typical findings in the area.

Hoarding.—A number of experiments, using various inbred and cross-bred mice, were conducted by Manosevitz and his associates on the determinants of hoarding (271, 272, 274, 275, 277, 278). A major consideration in these studies was the interaction between genetic and experiential factors. Strain differences were demonstrated in several studies (246, 271, 274, 275), and genetic analyses were performed to estimate various genetic parameters (278). The only significant heritable component was additivity although there was some suggestion that dominance might be present. Coefficients of genetic determination were estimated to be 37 percent in the F_2 generation and 49 percent in the parental lines (271). Heritability was estimated to vary between 29 and 54 percent (277). These results suggest that a partial answer to Bindra's (24) question "What makes rats hoard?" may be found in the genetic endowments of the subjects studied.

Several studies on hoarding have been concerned with various events, such as water immersion, neonatal irradiation, environmental enrichment, and aperiodic feeding experiences, that could affect hoarding performance, and these have been summarized by Manosevitz & Lindzey (278). These studies have provided some indication of the magnitude and general nature of genetic and environmental factors influencing hoarding. The frequent presence of genotype-treatment interactions (273) indicates that environmental experiences do not equally affect all genotypes.

Single-gene effects on motivation and temperament.—Studies relating single alleles of mice to temperament are those of Hawkins (158, 159) relating *yellow lethal* to reduced activity, Bartke & Wolff (19) implicating this same allele in estrus synchrony, and Iversen et al (194) correlating *hairless* to reduced spontaneous activity. Keeler and his colleagues are attempting to relate color changes in foxes, presumably under simple genetic control, to tameness and drug response (218, 220).

Recent efforts to identify single-gene influences on insect behavior are reviewed by Manning (270), Thiessen, Owen & Whitsett (388), and Wilcock (422). With rare exceptions the work has not been extensive, which is unfortunate in view of the fact that at least 675,000 species are available for

study—more than all other animal species combined. Rothenbuhler (341) summarizes his work on hygienic nest behavior of hymenoptera, demonstrating the influence of interaction of two genes—one influencing the opening of diseased larvae cells and the other influencing the removal of the diseased larvae. This work is a model of elegance and clearly demonstrates how a complex behavior can be resolved into individual parts and related to separate genetic units.

Animal social behavior.—A wide variety of behaviors are often considered to be “social,” including dominance, affiliation, and imprinting. However, these domains have not been extensively studied in a behavioral genetics context. This remains an area where important contributions could be made by behavioral geneticists. A number of significant investigations are identified in the volume edited by Glass (134), and another valuable source is the report by Scott & Fuller (356) of their extensive study of social behavior of dogs.

Strain differences in sociability have been reported (251), and these differences in affiliation do not merely reflect differences in activity, exploration, fearfulness (defecation), or freezing behavior. Social bonds formed through imprinting were studied using purebred and crossbred chicks (142), and population heterosis was shown for all three components of the imprinting response. Several studies on social dominance in chickens were reported (65, 304). A bidirectional selection study for high and low social dominance showed that large differences were produced in five generations of selection (66). Inbreeding reduced social dominance and aggressiveness (65). A further study of high and low socially dominant chickens showed that these differences may be due to differences in physiological responsiveness to social stimuli (304).

Social dominance in three inbred mouse strains (A, C3H/Bi, DBA/8) was measured by Lindzey, Manosevitz & Winston (247) using three behavioral indices of dominance: tube dominance, food competition, and a measure of spontaneous fighting (aggression). Large and reliable strain differences in all three measures were observed. It appears that tube dominance performance is inversely related to food competition and aggression. A recent study using a group of random bred mice showed that food competition performance was associated with general activity and emotionality (276).

A partial replication and extension of an earlier study (250) on social dominance was reported using different sublines of the original strains (249). Mice from three inbred strains (A/J, C3H/HeJ, DBA/1J) were tested in the tube test of social dominance. The A/J strain was the most socially dominant, defecated more in the open field, was less active in the open field, and was lower in total body weight. This study using different sublines did not replicate the social dominance differences observed earlier between the DBA and the C3H strains.

A series of studies by Fuller on the effects of isolation and the post-isola-

tion syndrome using beagles and terriers have shown large breed differences in the responses to social isolation (117, 121).

With an increasing emphasis on the importance of social behavior for evolution, we may expect to see a continued increase in the breadth and intensity of behavioral genetic research in this area.

NORMAL HUMAN PERSONALITY, INTERESTS, AND SOCIAL BEHAVIOR

During the past 5 years the total volume of available data on the inheritance of normal human personality, interests, and social behavior has probably at least tripled. It would be pleasant to say that this influx of new data has settled conclusively most of the outstanding questions in the field. It would also be untrue. Still, some progress has been made, even if in many cases the best we can say is that things are a lot harder than we thought. There are several good reviews and general discussions of work in this area (254, 395-397, 406, 407).

The largest body of new data is derived from questionnaire studies of adolescent US twins, including major studies by Vandenberg, Gottesman, Nichols, and Schoenfeldt. Other ages have not been neglected, however: Bruun and his associates have worked with adults in Finland, Koch and Scarr with US preschool and elementary school children, and Vandenberg's group and Freedman with US infant twins.

Studies of adolescent twins.—Gottesman reports data from a sample of 79 MZ and 68 DZ twin pairs from the Boston area on the Minnesota Multiphasic Psychological Inventory (MMPI) and the California Psychological Inventory (CPI) (135, 136). Vandenberg and his associates have given various twin samples in the Louisville area several personality and interest questionnaires, including the Myers-Briggs Type Indicator, the Stern Activities Index, the Comrey personality and attitude scales, and the Minnesota Vocational Interest Inventory (415, 416); their sample sizes range from 40 and 27 pairs to 111 and 90 pairs.

Nichols (301) mailed a battery of personality and interest inventories to a group of 1200 questionnaire-diagnosed MZ and DZ twins from among the large number of US high school juniors taking the nationwide National Merit Scholarship Qualifying Test in 1962, and received reasonably complete returns from both twins of about 70% of the group: 516 MZ and 334 DZ pairs. Included in the battery were the CPI, the Holland Vocational Preference Inventory, and a variety of other questionnaires, rating scales, and check lists. Schoenfeldt (354) obtained questionnaire diagnoses from 337 MZ and 156 DZ pairs from among the 400,000 high school students studied in Project TALENT, and reports results for a number of personality and interest factor measures derived from the original test battery used in that study.

Several fairly clear conclusions emerge from these investigations:

1. There is ample confirmation of the fact that pairs of identical twins tend on the average to be more similar in their scores on personality and interest

measures than do pairs of same-sex fraternal twins. Table 1 shows the median intraclass correlations obtained in various studies, for male and female identical and fraternal twin pairs. There is some variation, but overall a correlation of about .46 for identical and about .28 for fraternal pairs may be taken as representative. 2. Twin pairs of the two sexes show no striking difference in general similarity of personality traits. There is some indication of higher female correlations among the fraternal twins in Nichols' data, but the other samples do not support this. 3. The correlations for either identical or fraternal pairs on personality and interest measures are substantially lower than those typically found for ability measures. This is no doubt partly due to the lower reliabilities characteristic of personality measurement. But even corrected for attenuation, assuming typical reliabilities of around .75 for scales of this kind, the resulting correlations of .61 and .37 are lower than comparable figures from the ability domain. 4. Environment appears to carry somewhat greater weight than the genes in accounting for individual variation in personality traits. Holzinger's heritability coefficient based on the above corrected correlations is .38; Jensen's is .48. If one allows for a possible greater similarity of identical twin environments, the heritability estimates would be lower than this. 5. On the whole, there is little solid evidence that some personality and interest traits are consistently more influenced by the genes than others are. For example, there is little agreement in trait heritabilities across the two sexes. Thompson & Wilde (397) report the rank-order agreement between the heritability estimates for males and for females in eight studies in the literature, including most of those covered in the present review. They obtained Spearman rho's from .04 to .26, with a median of .15. Since a certain amount of purely artifactual consistency is to be expected, stemming from differences in the reliability of the scales, this level of agreement is not very impressive. Schoenfeldt (354) reports a similar lack of agreement between the sexes for the heritabilities of personality and interest factors in the Project TALENT data (not included in Thompson & Wilde's survey). These results might be interpreted as representing consistent sex differences in heritability but, alas, there is little empirical support for even this much consistency. Nichols (301) reports the correlations between the CPI heritabilities obtained in his sample and Gottesman's for males and females separately to be $-.22$ for males and $-.24$ for females. (As Nichols remarks: "Perhaps the best that can be said about these two correlations is that neither is significantly different from zero.") Thus, for the present, the most economical interpretation of the data would seem to be that while the genotype may have an appreciable effect on personality, the network of causal pathways between genotype and phenotype is so complex in this realm that the effect of genotype is spread almost evenly across the broad phenotypic measures that personality and interest questionnaires provide.

Younger and older twins.—Are things simpler, earlier? Several studies

from the period under review used younger twins. Scarr (345, 346, 348) used a small sample of girl pairs of elementary school age, 24 MZ and 28 DZ, and obtained experimenter's ratings and observations in standardized interview and experimental situations, as well as mother's ratings on the Adjective Check List (ACL). The median of the MZ and DZ correlations which were reported are .39 and .23 for measures and ratings based on the experiments and interviews, and .40 and .11 for those derived from the mothers' ACL. On the whole, allowing for sampling differences, these appear comparable to the figures cited earlier for high school age twins. Koch's detailed study of 5- and 6-year-old pairs (227) was chiefly focused on comparisons between different twin and non-twin groups, but she does report evidence that her identical pairs saw more resemblances between themselves and their twins than did fraternal pairs, and also that they shared more common experience and were psychologically closer in various respects.

Two studies report on even younger twins. Brown, Stafford & Vandenberg (39) provide some preliminary data on a group of 140 twin pairs being followed longitudinally. Only the 74 oldest pairs, aged 3 to 6 years, had been blood-typed at the time of the report. Eight variables were assessed, based on interviews with the mothers. The identical twins were more alike on seven of the eight, most strikingly so on feeding and sleeping problems. The more personality-like variables such as readiness to smile, temper, and dominance ("takes toys from twin") tended to show lower heritabilities. Freedman (113) worked with a small sample of 20 pairs during their first year. Diagnosis of zygosity at the end of the study revealed 9 to be identical and 11 fraternal. Films of the infants in standard situations were rated by judges, each judge seeing only one member of a given pair. On two items of prototypic social behavior, smiling and fear of a stranger, identical pairs were significantly more similar than fraternal.

These data on younger twins, taken together, tend to support the common observation that the greater resemblance of identical twins has early roots, but the data are insufficient to cast much light on the differential influence of heredity and environment on different traits, and thus to assist much in the interpretation of findings at later ages. A really large study of twins followed through the early years of life would be most welcome.

Partanen and his associates (311) report correlations on several personality traits in adult Finnish males, based on a questionnaire filled out by 157 MZ and 189 DZ pairs. The identical twin correlations on each of four personality measures were higher than the fraternal twin correlations; the median of their correlations (corrected for unreliability) were .54 and .36, or reasonably close to those found with US adolescent twins.

Twins reared apart.—In sharp contrast to the statistical studies described previously is the detailed case-history information presented by Juel-Nielsen (205) on 12 Danish pairs of identical twins reared apart from infancy (or early childhood, for some pairs). His personality measures were mainly pro-

jective tests, which are reported and analyzed on a case-by-case basis. In addition to these, the author reports his general impressions, based on interviews, of the areas in which the separated twins showed most and least resemblance in personality. According to him, the twins showed little resemblance in their modes of interpersonal interaction, their views on religious and social issues, their fields of interest, and the sort of persons they married. The twins also tended to be different in ambition, aggressiveness, and emotional control, and in matters of taste and dress. By contrast, the twins of a pair displayed marked resemblance in expressive movements:

TABLE 1. MEDIAN TWIN INTRACLAS CORRELATIONS FOR PERSONALITY AND INTEREST INVENTORY SCALES IN VARIOUS STUDIES

Inventory	Identical pairs		Fraternal pairs		Source
	Male	Female	Male	Female	
CPI	.53	.50	.25	.35	Nichols (301)
CPI	.45	.53	.30	.32	Gottesman*
VPI	.42	.42	.19	.28	Nichols (301)
TALENT	.44	.43	.33	.33	Schoenfeldt (354)
Comrey	.42	.42	.22	.19	Vandenberg et al (413)
Mean	.45	.46	.26	.29	

* Data reported in (136) in another form. Intraclass correlations supplied in a personal communication.

Juel-Nielsen reports being impressed repeatedly by similarities of gait, of carriage, of small gestures, of facial expression, of smiles and laughter, of tone of voice. A number of striking concordances in complaints of a psychosomatic type were also noted. In other respects their personalities tended to show an intermediate degree of resemblance, with a mixture of similarities and differences, often related to life events. A large amount⁹ of detailed information about these events is provided in almost 300 pages of case-history material on the twins. For any researcher in search of hypotheses, there is a mine of gold here.

PERSONALITY DISORDERS

Psychiatric genetics during the past 5 years could easily qualify for a whole review of its own. Consequently, the present discussion is highly selective. The psychoses, especially schizophrenia, have received the lion's share of research attention and will receive most space here.

Before becoming enmeshed in the details of particular disorders, we will mention a few items of broader interest. One is the volume on psychiatric illnesses of the handbook *Humangenetik* (20). Chapters by Zerbin-Rudin on the psychoses and Strömgren on the neuroses are of particular relevance here. Another item is a collection of Japanese studies in psychiatric genetics,

edited by Mitsuda (289). A third item is a recent book by Rosenthal (337) on genetic theory and abnormal behavior.

Finally, while the present review is organized under fairly conventional diagnostic labels, it should be made clear that how best to classify personality disorders is a very controversial matter among genetically oriented investigators in this area. Some use as their unit of analysis the presence of any psychiatric disorder. Others stick to classic Kraepelinian categories. Others subdivide these. Still others think in terms of a large number of highly specific genetic entities. To quote the population geneticist Morton on this point: "We may predict that in the future major genes (very likely as many different ones as have been identified in mental defect) will be found to account for many cases of mental disorders, leaving a component due to polygenes and nongenetic mechanisms that will defy analysis" (296, p. 183). Perhaps so. Only time (and the data) will tell.

Schizophrenia.—The landmark publication in this area during the past 5 years is clearly the Rosenthal & Kety volume on *The Transmission of Schizophrenia* (338), the proceedings of a 1967 conference at Dorado Beach, Puerto Rico, in which major US and European investigators from both genetic and environmentalist camps summarized their own research or reviewed relevant bodies of evidence. Included among the papers are reports of initial results from several important new studies, noted below. For some other major symposia and reviews in the past 5 years see References 61, 138, 334, 437. The puzzle of genes and schizophrenia is far from fitted together, but during the period under review some critical pieces took fairly clear shape.

The presence of a genetic predisposition to schizophrenia-like disorders may now be regarded as firmly established. Heston (172) matched 58 offspring of hospitalized schizophrenic mothers, given up for adoption at birth between 1915 and 1945, with 58 control adoptees of nonpsychotic parentage. Extensive follow-up beginning in 1964 located 47 of the experimental group and 50 of the control group, then at ages ranging from 20 to 50. Five members of the experimental group had become schizophrenic; none of the control group had. The frequency of schizophrenia in the experimental group was about as great as that expected among children of a schizophrenic mother reared in their own homes. There was a good deal of other assorted psychopathology in the experimental group as well: 24 other members of this group showed evidence of sociopathic personality, mental deficiency, or neurotic personality disorder, as opposed to a total of 9 such cases in the control group.

Support for Heston's main finding comes from studies of adopted children by Rosenthal, Kety, and their colleagues, preliminary results of which were reported at the Dorado Beach conference. The principal studies are being carried out in Denmark, because of the existence of superb medical and other social records there. One study (339) is essentially similar to Heston's, ex-

cept that children of either a psychotic mother or father are included, and in most cases the birth and adoption of the child occurred before the parent became overtly ill. At the time of report, 39 experimental subjects and 47 control adoptees had been examined by psychiatrists and psychologists completely blind as to their status. The results confirmed Heston's. Seven subjects diagnosed schizophrenic or borderline schizophrenic came from the experimental group, and only one (a borderline) from the control group. The Danish results, however, did not support Heston's finding of a good deal of criminal behavior, mental defect, and the like in the experimental group. Two other adoption studies by the same group (225, 418), one carried out in Denmark and one in the US, found considerable amounts of schizophrenia and related disorders in the biological families of adopted individuals who become schizophrenic, and little in their adoptive families. Another investigator, Karlsson (213), in a genealogical study in Iceland, found similar results using the biological and foster siblings of adopted schizophrenics. He also reports adoption data consistent with Heston's and Rosenthal's.

Taken together, these studies give clear confirmation of some degree of inherited predisposition to schizophrenia-like disorders. However, they suggest also that this predisposition does not always become manifest in clinical schizophrenia. More direct evidence on this point comes from the studies of monozygotic twins to be considered next.

Early studies found quite a high incidence of schizophrenia in the co-twins of MZ schizophrenic index cases (Kallmann's figure of 69% is representative). A trickle of lower concordance figures beginning to appear 5 years ago is now a flood. The difference at least partly reflects a difference in method. Most of the early studies started with severe hospitalized schizophrenics; the newer studies have searched whole populations or started with milder psychiatric cases. Tienari (399) in Finland found only one clearly psychotic co-twin of 16 schizophrenics who were members of identical twin pairs, or 6% concordance. Adding four cases with some borderline schizophrenic symptoms raises the figure to 31%. Kringlen (229-231) in Norway, with 55 MZ pairs found concordance for 25%-38%, depending on definition of cases. Fischer et al (108) in Denmark found 25 pairs with 36%-56% concordant. Gottesman & Shields (139) in England, with a psychiatric outpatient series found 42% of 24 MZ pairs concordant. Finally, reports based on the large US veterans twin sample (319) indicate that in this group of twins (from which any early psychotics would be excluded), out of 80 MZ pairs with at least one member diagnosed schizophrenic, in only 11, or 14%, was the second twin also schizophrenic. The twins, age 38 to 48 when studied, were nearing the end of the risk period for schizophrenia.

Taking all these results together, it is clear that the genetic predisposition to schizophrenia is of far less than perfect penetrance, since the same genotype which leads to schizophrenia in the one MZ twin fails to do so in the other at least as often as not. Thus while a predisposition seems to be inherited, environment (in some form) obviously plays a large role in determining whether it will be expressed as clinical schizophrenia.

It should not be concluded that the nonschizophrenic MZ co-twins of schizophrenics are perfectly normal. Some degree of schizoid personality, neuroticism, or general eccentricity is commonly found in such individuals, although a few apparently normal co-twins have been reported (229, 318, 399). It is tempting to consider these personality deviations as milder manifestations of the schizophrenic genotype (173, 315), but some could conceivably reflect the effects of intimate association with the psychotic or prepsychotic twin. A small amount of critical data on this point is supplied by Mitsuda (290), who located in Japan eight cases of that very rare species identical twin pairs reared apart from infancy in which at least one member has become schizophrenic. Five of the eight co-twins were schizophrenic, two were said to be schizoid, and one was normal. Again, it appears that certain genotypes predispose to schizophrenia, but may be expressed in milder personality deviation, or even be compatible with normality.

Discordant MZ pairs have been examined in several studies in the hope of identifying the environmental factors that have led to the difference in outcome (229, 318, 399). No conclusive results seem yet to have emerged. A rather characteristic finding is that the schizophrenic-to-be is the more submissive and introverted of the pair, but whether this is a contributory factor or merely an early symptom of the schizophrenia is not yet clear.

Another promising approach to investigating the expression of the schizophrenic genotype has been taken by Mednick & Schulsinger (284, 285). These investigators are following 207 offspring of Danish schizophrenic mothers. All subjects were given extensive initial tests and interviews. Perhaps half of these children, in their teens at the start of the study, are expected eventually to show some degree of socially deviant behavior, with about 30 becoming schizophrenic. After 5 years of follow-up, approximately 20 subjects have shown some more or less severely deviant behavior, although none is yet unambiguously schizophrenic. Further developments in this study will be awaited with great interest.

There is no evidence that anyone has given up his pet genetic hypothesis about schizophrenia during the past 5 years. Eloquent voices have been raised on behalf of a single partially dominant gene (97, 371), a dominant and a recessive gene (213), two recessive genes (47), a polygenic system (140), and some sort of generalized genetic disharmony (25). The notion of schizophrenia as a heterogeneous group of genetic diseases has also been popular (102, 290). Along this last line, Hanhart (153) has urged the study of schizophrenia in isolated human groups, where the chances of finding just one genetic variety would presumably be enhanced. He reports data from a Swiss isolate with considerable inbreeding and a relatively high frequency of schizophrenia, and suspects a recessive gene with reduced penetrance.

Regrettably, the earlier cited evidence from discordant MZ twins concerning the varied and uncertain manifestation of the schizophrenic genotype(s) suggests that, barring some biochemical breakthrough, none of the above genetic hypotheses is in imminent danger of extinction.

Since schizophrenics tend to have fewer offspring than nonschizophrenics,

the gene(s) involved should be selected against. Yet they are apparently much more frequent than can be explained plausibly by new mutations. It has been suggested that the schizophrenic gene(s) in other contexts may confer a reproductive advantage (191). This idea has been widely discussed in the period under review (25, 103, 135, 207, 213, 232) but no solid evidence has yet emerged. Some data *have* been reported suggesting that the reproductive disadvantage of schizophrenics is rapidly diminishing (104), due largely to modern treatment methods which allow the schizophrenic to spend more time in the community. The social implications of this trend deserve attention.

Affective psychoses.—While genetic research with the affective psychoses has been somewhat less active than that with schizophrenia, there have been several substantial investigations and some good reviews and symposia (62, 437). Perhaps the most striking results from a genetic standpoint are those of Perris (313). He investigated the hypothesis of two different genetic entities in the affective psychoses, one marked by both manic and depressive phases ("bipolar"), and the other by recurrent depressive (or manic) phases only ("unipolar"). Among the relatives of 138 bipolar index cases he found 58 bipolar and 3 unipolar cases, and among the relatives of 139 unipolar depressive cases he found 44 unipolar and only 2 bipolar cases—a remarkably clear separation. (He only found 17 unipolar manic index cases, and they did not yield very clearcut results.) The two varieties of affective psychosis differed in several respects: most notably in age of onset, with unipolar cases occurring later. In addition, the risk of psychosis among relatives was lower for the unipolar cases. In general the study appears to have been carefully done, but Perris did all his own diagnoses, and it is difficult to rule out the possibility of some contamination, particularly in the diagnosis of the relatives.

In a similar study in Switzerland, Angst (15) found most bipolar illness occurred among the relatives of bipolar probands, in agreement with Perris; however, unipolar illness occurred fairly often among the relatives of both kinds of probands. Angst also confirms Perris's finding of a lower psychiatric risk among relatives of unipolar probands, as does Asano (16), in a Japanese sample restricted to cases with an early age of onset (under 40). In Great Britain, Hopkinson & Ley (183) found a lower risk of affective disorders in relatives of probands with relatively late illness, and some tendency toward a bimodal distribution of age of onset. However, in a substantial US study of affective psychosis by Winokur and his associates (427) there was no difference in average age of onset between familial and nonfamilial cases of affective disorder, which is somewhat out of line with the other studies. Winokur's group did find that most of their cases displaying manic symptoms fell in a group with highly positive family histories (426), which is consistent with Perris's results.

Thus on the whole, despite some inconsistencies, there appears to be a

fair amount of support for the hypothesis of two fairly distinct entities among the affective psychoses, one marked by the presence of both mania and depression, a higher risk in relatives, and earlier onset, and the other by recurrent depression, less risk in relatives, and later onset. Winokur proposes that a dominant form of genetic transmission may be involved in the first type (426). However, Slater & Tsuang (372) failed to find support for dominant transmission in manic-depressive families, using a method based on the frequency with which cases are found only on one side of the family.

Psychoneuroses and character disorders.—Shields & Slater (364) report higher concordance among identical than among fraternal twin pairs for anxiety states and for personality disorders, but not for other neuroses. Mitsuda and his co-workers (291) found that none of 56 cases of neurosis diagnosed as hysteria or depressive reaction had cases of schizophrenia among their close relatives, as opposed to 23 such families among 100 cases of neurosis diagnosed as anxiety reaction, obsessional state, or oversensitivity. The possibility must be considered that some of the latter group represent milder expressions of genotypes which might under other conditions have led to schizophrenia.

Guze (149, 432) reports an elevated incidence of hysteria in female relatives of hysterics, and in male relatives an excess of other psychiatric disorders, mostly sociopathy and alcoholism. The incidence of the latter disorders was even higher in the *husbands* of his index cases, suggesting the possibility that it is hysterical females, not hysterical genes, that drive men to drink. Substantially higher identical than fraternal twin concordance for criminality was reported from Denmark (157), while studies of obsessional neurosis yielded somewhat mixed results (133, 193, 335, 344). Some limited new twin data on homosexuality suggests the involvement of both genetic and environmental factors (174). The US veterans twin sample gave a much lower MZ/DZ concordance ratio for psychoneurosis than for schizophrenia (319). On the whole, a mixed bag, with some evidence of the importance of the genes, but every reason to suppose that the environment plays a major role as well in these disorders.

Single-gene and chromosomal defects.—Much less is known about the influence of genetic and chromosomal anomalies on personality and social behavior than on intelligence. Two exceptions to this generalization will be discussed briefly. One concerns genetic and chromosomal effects on sex role identification. The other concerns the XYY chromosome constitution and criminal behavior.

Representative of work on genetic and chromosomal sex is the research of Money and his associates (294). A particularly illuminating instance is provided by the testicular feminizing syndrome (292). This condition, believed to be due to a single gene, appears to involve an insensitivity of the body cells to male hormones. Affected individuals, genetic males, have (internal)

male gonads, but the androgens secreted by the testicular tissue do not have their normal masculinizing effects, and the individuals develop as phenotypic females, except that they do not have female internal reproductive organs, do not menstruate, and are of course infertile. The interesting fact about such patients from our present standpoint is that their sex role behavior and attitudes appear to be entirely feminine: as little girls they show no signs of tomboyishness, and when they grow up they fall in love with masculine males, get married, and settle down to cheerful domesticity. They also tend to show the typical feminine pattern of cognitive abilities, with verbal abilities superior to perceptual-spatial ones (282). Thus their genetic maleness appears to have no direct psychological effects; presumably it normally acts only via the male sex hormones. However, there may be a direct effect of Y chromosome genes in growth, since these girls tend to be somewhat taller than average.

Much research and legal and popular discussion have stemmed from the reports by Casey (54) and Jacobs (196) of a number of men with an extra Y chromosome found in special security institutions for mentally subnormal men with aggressive and criminal tendencies. It was noted that many of these individuals were exceptionally tall, and soon investigators on several continents were busily examining the chromosomes of tall prisoners and mental patients and finding some cases of XYY. Court Brown (64) and Kessler & Moos (224) provide good reviews of these studies, and Borgaonkar (29) provides an extensive bibliography. On the whole, it appears that if one surveys 100 penitentiary inmates over 6 feet tall he can expect to find two or three men with an XYY karyotype. It is clear that this is well above general population levels: Court Brown found one XYY male in a survey of 1185 male births, and none in an industrial population of 371 adult males selected for a height of 6 feet or more (64). Nine XYY cases were found among 8215 males karyotyped in 15 French laboratories for a variety of reasons (324). Three cases in approximately 2000 male births were found in a US study (256). A somewhat higher figure of 4 in 1066 male births has been reported in one study (361), but even this is far below typical findings in criminal populations.

Obviously, only a small fraction of serious criminals have an extra Y chromosome. Furthermore, neither tallness nor mental subnormality nor criminal tendencies invariably accompany an XYY karyotype. Indeed it has recently been proposed (50) that there are at least two quite different XYY syndromes, one marked by various genital anomalies, but not by tallness or psychopathic behavior, and the other by tallness and psychopathic tendencies, along with normal genital development. Judging from estimated population frequencies, most XYY males fit neither syndrome, but lead ordinary lives in society (162). One such individual, discovered by chance (421), was 5'11½" tall, had an IQ of 97, was physically unremarkable, and had exhibited a cheerful disposition and a mild and law-abiding temperament since childhood.

EVOLUTION

GENE DIVERSITY AND ADAPTATION

Many forms of behavior that are critical for individual survival and population adaptation are under genetic control. Mating speed and duration of mating in *Drosophila* species show extensive genetic variation (215, 309, 310), and respond to directional selection (223, 258). Similar genetic determination is evident for mating ability in chickens (369), aggressive behavior in mice (212, 233, 239), nest building in rabbits (435), and sex behavior in mice (264-267, 400).

Dobzhansky and his collaborators have been the most effective in identifying genetic variation in *Drosophila* and relating this variation to population survival. Using mass screening devices developed by Hirsch, these investigators have identified positive and negative geotaxis and phototaxis in many strains of *D. pseudoobscura* and *D. persimilis* from various geographic localities (375). Some strains are positive, others neutral, and still others negative. *D. pseudoobscura* has been selected for high and low characteristics on both of these responses (84, 85, 88). Realized heritability of both traits was extremely low—8 to 10% for the phototactic and 2 to 3% for the geotactic behavior. In spite of this, clear separation of selected lines was obtained after 16 generations, and when selection was relaxed the populations again converged, almost as rapidly as they were separated (88). Apparently geotaxis and phototaxis neutrality are the optimal responses among these populations—any radical deviation in either direction is counterbalanced by natural selection. When only a few “migrants” are permitted between populations separated for their responses to gravity and light, the migrants will alter the recipient populations in the direction of the donor populations, even though phenotypically the migrants are the most deviant individuals of the donor groups (87, 89). In other words, the phenotype of the migrants is not a good index of their genetic architecture, in accord with the finding of low heritabilities for these traits. Dobzhansky views these experiments as models of social mobility that may have application to human populations.

One explanation for the disproportionate influence of rare genotypes on population parameters is found in recent studies by Ehrman and associates (93, 94). Rare *Drosophila* males mate more frequently than males that are common. The phenomenon has now been observed in strains of *D. melanogaster*, *D. pseudoobscura*, *D. persimilis*, *D. tropicalis*, *D. willistoni*, and *D. equinoxialis*, and in strains carrying different chromosome arrangements (AR and CH), strains selected for different behavior, and in those containing mutant markers. For whatever reason, rare male genotypes are reproductively more fit until the numbers of each in the population are equal, at which time mating becomes random. The effect is less pronounced for females. Obviously this mechanism could lead to balanced polymorphism without heterozygous advantage. The exact sensory mechanisms underlying

the phenomenon are unknown, although olfactory and tactual cues have been implicated (93).

ETHOLOGICAL ISOLATION

Genetic groups, especially sympatric populations, may become reproductively isolated because of behavioral differences. This is in fact one basis for speciation and is of increasing interest to behavior geneticists. Isolation may occur because of differential selection pressure in a diversified environment (131, 141, 243), because of divergent evolution from founding stocks that contain few individuals with unique genotypes (184, 185, 308), because of inadvertent selection during domestication (321), because of differences in reproductive behavior (222) and physiologies (40, 281), because of different capacities for food utilization under population stress (237), and because of assortative mating for various traits (13, 86). Obviously the various reasons for isolation are not independent and must be studied in their interactions. Like other traits, isolating mechanisms can come under the control of selection in the laboratory (77, 221). As yet speciation has not occurred under laboratory conditions, but it might be a reasonable goal.

While isolating mechanisms are better studied in organisms with a quick turnover of generations, some work with higher organisms shows promise of revealing critical ethological factors. Bruell (44) has proposed designs for evaluating the effects of inbreeding on wild populations. Emphases are on comparisons between sympatric mouse populations in the wild and in the laboratory. Selander & Yang (359) have studied several rural populations of domestic mice using blood esterases and hemoglobin as genetic markers to evaluate geographic variations in polymorphisms. Significantly, isolated breeding populations with characteristic gene frequencies appear even within individual barns with no physical barriers. These "tribes," "demes," and other breeding units are apparently isolated by male territorial and aggressive behavior. Surprisingly, up to 25% inbreeding is tolerated within subgroups. Some investigators believe that research into ethological isolating devices and modes of adaptation is of paramount importance for the continuing vigor of behavioral genetics (280, 385).

HETEROSIS

The terms heterosis or hybrid vigor are used to refer to the observation that crossbred F_1 offspring from two inbred parental lines either deviate significantly from the expected midparent value or exceed the extreme parental value. Both definitions have been used by behavioral geneticists. There are several competing theories of heterosis, and behavioral geneticists have done little to resolve these issues. Hybrid vigor is negatively associated with inbreeding or reduced genetic variation; indeed the opposite of hybrid vigor is called inbreeding depression. Falconer (107) suggests that when inbred lines are crossed the F_1 offspring will show heterosis for those char-

acters that showed inbreeding depression in the inbred lines. It appears that inbreeding allows more deleterious recessive genes to be expressed, and thus crossing, which increases genetic heterozygosity, should mask more of the recessive deleterious genes (245), and this may be what one observes in heterosis.

Hybrid vigor has intrigued geneticists for many years, and in agriculture and animal breeding extensive and valuable economic use has been made of this phenomenon. However, at this time the amount of research devoted directly to investigating *behavioral* heterosis has not been extensive. Reviews of this literature may be found in a chapter by Bruell (42) and Lindzey's American Psychological Association presidential address (245).

Heterosis has been observed for many phenotypes including morphological and reproductive characters such as: litter size, longevity, body size, egg size and production, and disease resistance, using a variety of organisms such as: chickens, mice, rats, silkworms, honeybees, and *Drosophila* (245). Most studies that have shown evidence for behavioral heterosis have been concerned with simple nonsocial responses such as appetitive and aversive learning (59, 429), activity (41, 42, 295), emotionality (43), and exploration (41). A study by Barnett & Scott (17) was concerned with nest building, food and wood gnawing, and activity, responses that appear to be closely related to adaptation. The investigators found evidence for "behavioural vigour" or heterosis in activity, gnawing, and nest building. In studies of heterosis, both behavioral and nonbehavioral, those characteristics that show heterosis seem to be associated with adaptation to the environment, natural selection, and thus evolution. Regrettably, social behavior, which from an evolutionary perspective is most important, seems rarely to have been the object of behavioral heterosis studies.

Evidence for behavioral inbreeding depression in man has been discussed by Spuhler (376), but evidence for behavioral heterosis in man is difficult to obtain. An interesting study by Hulse (189) provides some limited evidence for morphological heterosis in humans. He found that offspring from marriages between members of different Swiss villages were taller and more robust than offspring of within-village marriages. Lindzey (245) has discussed the evidence for inbreeding depression as a result of incestuous matings and consanguineous marriages. On the basis of the results presented in that review one could reasonably infer that heterosis for human traits, behavioral and nonbehavioral, should be observable under appropriate circumstances.

A major publication on behavioral heterosis during the period under review was Bruell's chapter (42). In it he reviewed several diallel studies of heterosis, and discussed genetic mechanisms that may underlie heterosis, and the relationship between evolution and heterosis. He also provided several suggestions for the design of future experiments on behavioral heterosis. In another important paper, Parsons (307) reviewed several studies that used mice and *Drosophila*, and examined the relationship between behavioral heterosis and homeostasis, i.e. reduced variability in hybrids. It

appears that heterosis is often associated with homeostasis and that this association is stronger when the hybrid crosses are between unrelated strains. Only a few studies (163, 164, 428, 429) have directly investigated the ability of hybrid organisms to withstand various environmental changes or stresses, which has been called buffering (235). Additional studies of this problem would be a valuable contribution to the study of behavioral homeostasis and heterosis. Since heterosis and inbreeding depression are so closely related to evolution it can be expected that behavioral geneticists in the future will devote considerable experimental effort to these phenomena.

RESEARCH METHODOLOGY

In this section various techniques used in behavioral genetic analyses are briefly discussed and a few comments are made regarding trends in the use of these techniques. Examples of substantive findings yielded by these methods have been presented throughout this review.

SINGLE-GENE TECHNIQUES

Single-gene methodology is currently drawing increased attention, and the mouse has established itself as the chief species for study. Three major reviews have appeared (261, 388, 422) and several discussions point to the usefulness of single gene analyses (1, 269, 288, 386). By substituting single genes within a population, an investigator can preset biochemical substrate, relate behavioral effects to chromosome positions, and correlate allelic variations in physiology and morphology to phenotypic components of behavior.

In a critique, Wilcock (422) described the major techniques of unifactorial study and stipulated their limitations. The primary methods of assessment are (a) to trace backward in development a gene mutation until its effects vanish, thus indicating the point of genetic influence (e.g. 384); (b) to compare single-gene variations at a particular point in time and assess the differences in the phenotype (e.g. 1); (c) to screen alleles on a battery of behavioral tests and look for patterns and specificity of influence (e.g. 388); and (d) to resolve biometrical traits into their separate genetic components (e.g. 176, 329, 341, 393). Each approach has its special advantages as well as deficiencies.

Genes of interest are maintained in stocks of animals by preserving spontaneous mutations in inbred lines, by introducing mutations in inbred lines with repeated backcrossing from a mutant source, and by crossing mutant and nonmutant stocks and recovering segregants in F_2 or other heterotypic generations (144). The first method reduces the possibility of linked genes affecting the phenotype, while the second and especially the third method allow linked genes to influence the results. Several catalogs of single genes are available (e.g. 257, 379), and an annotated bibliography of mutant mice has been compiled (367). In the mouse, well over 300 mutant genes occupying more than 250 loci are known (143). Of these at least 55 are non-lethal coat color genes (433), in many respects ideal for behavioral study.

Moreover, over 100 neurological mutants are recognized in the mouse (367) affecting almost every conceivable biological function relevant to behavior. In *Drosophila* at least 479 loci have been identified (12), and in man a minimum of 123 loci have been identified on the sex chromosome and more than 1400 loci are noted for non-sex chromosomes (268). Here, then, is the genetic raw material for investigation, especially abundant in mouse, *Drosophila*, and man.

Relatively little of the quantity and diversity of single genes has been explored experimentally. Investigations of neurological mutants in mice have been most productive, and we will focus upon these here. Other significant investigations are cited at the end of this section.

Neurological mutants.—Neurological mutant mice can be classified roughly according to (a) the area of the central nervous system affected (367); (b) the level of defect (e.g. 365); (c) the age of onset (e.g. 111); or (d) the type of behavior involved (e.g. 111, 248, 384). It seems that most known mutant disorders arise during early development (e.g. before 30 days of age) and are associated with smaller brain and body size and are related to locomotor disabilities (especially swimming). Often the defects are pleiotropic to changes in coat color and spotting.

The variety of neurological mutants affecting balance and locomotion is very great. A partial listing can be found in McClearn & Meredith (261) and a more or less complete cataloging of such mutants is available (367). New discoveries and studies include *dancer*, *ducky*, *dystonia musculorum*, *jimpy*, *jittery*, *Nijmegen waltzer*, *pallid*, *pirouette*, *quaking*, *reeler*, *retinal degeneration*, *rotating*, *shambling*, *Snell's waltzer*, *twirler*, *wabblers-lethal*, *waltzer*, and *whirler* (2, 80, 111, 248, 343, 366).

Screening techniques.—Somewhat paradoxically, methods devised to understand polygenetically controlled behavior have introduced new means of studying and classifying single-gene differences. Hirsch (176) has devised elegant techniques of mass screening and genetic analysis that function in this way. He and his colleagues (176, 179, 181, 186, 316) have used genetic markers, chromosomal inversions, and balanced lethal systems to study geotaxis in *Drosophila*. As a result, negative and positive geotactic behaviors have been related to three of the four fly chromosomes. This refinement, as Hirsch and others see it, is a prelude to resolving chromosomal influence into smaller and smaller units, much as Thoday (393) and others have done for bristle number in *Drosophila*. Eventually, individual genes may be isolated following this strategy.

Seymour Benzer (22), a Nobel laureate in genetics, has amplified Hirsch's mass screening techniques to study single-gene mutations in *D. melanogaster*. He induces the mutations with ethyl methane sulfonate (EMS) and screens the mutants and normals on successive light stimulus tests. The technique, which Benzer calls "countercurrent distribution," may offer a useful method

for fractionating populations according to genetic diversity on a number of sensory-motor dimensions. Benzer's technique has been used by others (211) to study induced neurological mutants in *D. melanogaster*. Of the four dominant mutants examined (mapped on the X chromosome) which cause shaking following etherization, marked activity, increased wing scissoring and increased fly specking (defecation) following disturbance, none reacted in any abnormal way to a light gradient.

An elegant yet simple single-gene method of great potential has been developed by Adler (6) and his associates to study chemotaxis in *E. coli* bacteria. A capillary tube containing a solution of a chemical attractant is pushed into a suspension of bacteria on a slide and the number of bacteria attracted is counted. Using this straightforward technique combined with specific monounit mutants, Adler was able to conclude that the periphery of *E. coli* contains at least five chemoreceptors which direct movements of flagella toward different chemical substances. These receptors respond to galactose, glucose, ribose, aspartate, and serine—chemicals of interest to bacteria. The receptor mechanisms are independent of metabolic processes needed for survival and membrane transport systems. The methodology could be applied to the study of other sensory systems. In general the research steps of Hirsch, Benzer, Adler, and others significantly advance the concepts of taxes and innate releasing mechanisms established by Loeb and European ethologists and promise new and exciting developments in the understanding of single-gene processes.

DESIGNS AND MODELS

Along with the growing use of single-gene analyses and mutants as techniques in behavioral genetic analyses there is continued use of a variety of crossing techniques. Genetic analyses using two inbred strains (P_1 , P_2), F_1 , F_2 , and backcrosses (B_1 , B_2) to estimate components of variation, heritability, and other genetic parameters have been used by a number of investigators (e.g. 148, 278, 299, 394). A computer program which can be used to estimate the various genetic parameters and test the assumptions of the classical model is available (214). In studies reported by Newell (299) and DeFries & Hegmann (74) various biometrical analyses were used and compared. Such comparative analyses of biometrical methods should be useful in future behavioral genetic research. In a study of audiogenic seizures (60) three additional segregating generations, B_1F_1 , B_2F_1 , B_1B_2 , were added to the usual generations. A goodness of fit test was used to assess the congruence between expected and observed proportions of seizures in each generation.

The "Tryon effect."—The absence of greater phenotypic variance in the segregating F_2 generation compared to the F_1 generation was discussed by Hirsch (178), and he suggested that this was due to inadequate sampling of the spectrum of possible F_2 genotypes. Tellegen (383) has suggested that as

long as sampling of the F_2 subjects is random, an unbiased estimate of the population variance should be obtained. He has also shown that as n , the number of relevant genes, increases, the probability of observing the Tryon effect increases, but as the degree of genetic variation relative to environmental variation increases, the probability that the Tryon effect will be observed decreases. The absence of the Tryon effect was reported by DeFries & Hegmann (74) and their discussion of this effect is similar to that provided by Tellegen.

The diallel cross.—In the past 5 years there has been increased interest in the diallel cross as a technique in behavioral genetics. A variety of organisms have been studied with the diallel method including rats (34, 36), mice (43, 59, 164), and fruit flies (114, 307). Broadhurst (34) provides an introduction to the diallel cross and discusses the advantages, limitations, procedures, and inferences that can be made using the technique. He illustrates the design from his own work on open-field behavior in rats. It is likely that this technique may be used more frequently in the future; indeed, Henderson (164) has suggested that generally the use of inbred or random-bred mice should be "abandoned" and replaced by diallel designs.

However, other promising new alternatives to the diallel cross are being proposed. The quantitatively oriented behavioral geneticist should find several papers by the Birmingham biometricians of considerable interest (e.g. 45, 202, 204, 216, 283).

Although selection is probably the technique most generally identified with behavior geneticists, it has not been used extensively in recent years. Broadhurst (36) and Roberts (328) have discussed some of the reasons for this. Broadhurst (36), who in his own work has used artificial selection extensively, writes "... despite this widespread interest [in selection experiments] the harvest yielded by so much work has been relatively barren" (p. 807). Selection experiments have been carried out using diverse behavioral phenotypes and organisms, e.g. maze learning, avoidance learning, activity, alcohol consumption, and emotionality in rats; aggressiveness in mice; geotaxis, phototaxis, and mating speed in *Drosophila*. Roberts (328) provides a discussion of genetic concepts that underlie selection studies.

It should be noted that some interest is now being shown in using wild *Mus musculus* (44) and other noninbred and nondomesticated mice. Bruell provides cogent reasons for increased concern with organisms that are not peculiar to the laboratory. He has argued that domesticated inbred mice, commonly used, represent a highly selective sample of the total genetic variation of mice. As behavioral geneticists become more interested in evolution and behavioral population genetics, continued use of domesticated inbred mouse strains will become more restricting.

Genotype x environment interaction.—The frequent observation of genotype x environment (or treatment) interactions, in a wide variety of be-

havioral traits, raises formidable problems in the design and strategy of research and interpretation of research findings. One may view the ubiquitous $G \times E$ interactions as either trivial or else as a basic challenge to psychology as a science with general laws of behavior. Examination of these problems and some solutions and research strategies can be found in the reviews by Henderson (165), Manosevitz (273), and Vale & Vale (403).

Psychologists have generally been concerned with the effects of specific treatments (main effects) upon behavior, while behavioral geneticists have typically been concerned with the effects of genes upon behavior. While some investigators have been directly concerned with genotype $\times$ environment or treatment interactions, there have been few systematic programs of research on such interactions. Interested readers should find the following an adequate starting point for further inquiry into this area (72, 120, 132, 152, 164, 165, 177, 273, 307, 336, 378, 395, 396). Suggestions have been made to show how uniformity within an array of complex interactions may be found (165, 403), and a scheme for classifying the various kinds of interactions has been reviewed by Manosevitz (273). Suggestions found in the paper by Vale & Vale (403) for searching for the "underlying variables," "mechanisms," or "processes" of the interaction should stimulate not only a new look at $G \times E$ interactions but also systematic studies in which these interactions are investigated and efforts are made to identify their bases.

Genotype $\times$ age interaction.—Behavioral genetic research on genetic variation and development has been concerned with (a) genetic differences in developmental rate; (b) interactions between genetic factors and age of subjects at time of testing; and (c) interaction between genetic variation and early experience treatments. Genetic differences in developmental rate and behavioral stability have been studied by comparing strains or mutant stocks with respect to a variety of behaviors (38, 83, 112, 358, 417). Generally it has been shown that genetic factors are important determinants of developmental rate and behavioral stability.

The importance of genotype $\times$ age interactions has been emphasized by Henderson (166), and interested readers should find his review of these studies helpful. He has suggested that investigators become alert to detecting genotype $\times$ age $\times$ treatment interactions in their research. Studies on the effects of early experiences and their interaction with genetic factors as determinants of adult behavior have been studied by several investigators (e.g. 164, 275). Generally these studies show that responses to early experience and the course of subsequent development of the organism are partly determined by genetic factors and the age of the organism when he is exposed to the treatment.

Twin and family studies.—A number of methodological developments in human behavior genetics are noted elsewhere in this review in connection with substantive studies. The present section will discuss some general

methodological trends, and some particular issues of method not covered elsewhere. The reader's attention is also invited to a general discussion of twin methodology by Allen (9), and to a symposium on the use of computers in human genetics (370).

An authoritative discussion of twin concordance rates is presented by Allen, Harvald & Shields (10). They distinguish between "pairwise" and "probandwise" concordance rates. The former (which is the definition used elsewhere in the present review) refers to the proportion of affected pairs that are concordant; the latter, to the probability of having an affected co-twin. They note that the pairwise rate in a sample is a biased estimator of the corresponding population parameter, and present a method of correction. A related discussion of twin models is presented by Gedda & Brenci (130).

A striking (and welcome) trend in twin studies is toward the use of large samples which are essential for quantitative estimates of heritabilities, or for multivariate analyses. A 1965 survey by the World Health Organization (434) counted 23 current or planned twin studies in the areas of "psychiatry, psychology, or normal development" in the US, Europe, and Japan. Seventeen of these involve over 500 twin pairs, and six over 2000. Such large-scale studies usually require questionnaire diagnosis of twin zygosity, based on physical characteristics, mistaken identity, and the like. Several recent validations of questionnaires against blood typing methods have been undertaken, with reassuring results. Nichols & Bilbro (302), with the US National Merit Scholarship sample, found agreement between a mail questionnaire and blood diagnosis in 78 of 84 blood-tested pairs, or 93%. Hauge and his co-workers (157) with the Danish twin sample found agreement on 313 of 335 pairs, again about 93%. A study with a sample of twins from among US veterans (195) achieved approximately the same level of agreement for a mail questionnaire against a combined serological and morphological criterion, for 232 pairs. Since blood tests themselves are typically 97-98% accurate, and since the errors are probably reasonably independent, this suggests that the questionnaires are actually achieving around a 95% level of diagnostic accuracy, which should be quite satisfactory for most large-scale studies.

A number of writers have discussed the estimation of heritability coefficients from twin and family data (67, 140, 199, 200, 254, 300, 354). As several have pointed out, this is only a special case of the more general problem of assigning the phenotypic variance of a trait in some population to various genetic and environmental sources. The more recent approaches take into account (to varying degrees) such factors as assortative mating, nonadditive genetic variance, heredity-environment correlation and interaction, and (for twins) the possible greater similarity of environments between members of identical pairs than between members of fraternal pairs. Since it is clear that one cannot estimate all of these parameters from twins alone, more complex designs involving other family members as well are

being explored (98). Cattell's pioneering effort in this direction, *Multiple Abstract Variance Analysis*, has been criticized (253), and elaborated (55). In a major paper, Jinks & Fulker (201) examine thoroughly the whole area of quantitative estimates of variance components in human behavior, from a biometrical genetics point of view. While it is possible to find fault with the assumptions underlying any particular one of these techniques, it is clear that there are many options open in designing quantitative studies of the genetic and environmental influences on human traits, options which represent special cases of a very general approach.

Two factors mentioned above which have come in for special attention recently are assortative mating and the equivalence of identical and fraternal twin environments. Anyone wanting to incorporate assortative mating into a quantitative model will wish to consult a paper on its genetic implications by Crow & Felsenstein (68), which provides a simple Wrightian approach to Fisher's classic work. Other papers review assortative mating with respect to education (226), IQ (128), and physical characteristics (377) and discuss theories of mate selection (92). Work on assortative mating with respect to normal and neurotic personality traits has also been appearing (46, 56, 154, 228).

The assumption that fraternal twins have just as similar environments as do identical twins is basic to many methods of estimating heritability from twin data. It has been a major focus of attack for critics of such methods. Additional ammunition for the critics is provided in several recent studies showing that compared to same-sex fraternal pairs, identical twin pairs have more close friends in common (373), play together more as children (227), and are responded to more similarly by teachers and peers (360). Some evidence for the defense is reported by Scarr (347), who suggests that, at any rate, parents' identification of twins as identical or fraternal may not be crucial. She looked at cases where parents were mistaken concerning the zygosity of their twins, and found that this seemed to make little difference in their resemblance. There were only 11 mistaken pairs in her sample, however, so this approach deserves repetition on a larger scale.

In general, the causal sequence in development is hard to unravel. Do identical twin pairs play together more because they are more alike, or are they more alike because they play together more? Studies of separated twins, or longitudinal studies, should be able to get some leverage on this distinction. A statistical approach is also possible, if one assumes a constant increase in environmental similarity relevant to several traits of differing heritability (254). Stability of relationships across cultures may also provide a clue (146, 147).

Potentially powerful tools for tearing apart complex causal influences are presented by multivariate statistical methods. For a sampling of such techniques applied to human behavior-genetic problems, in the realm of abilities, the reader is referred to the concluding section of Vandenberg's

recent volume (410); such techniques have also been applied in the personality realm (252, 311, 398).

IN CONCLUSION

It seems evident that the field of behavioral genetics has displayed an accelerating rate of growth that is matched by few psychological specialties. It provides a major point of intersection between psychology and the biological sciences and in recent years has involved extensive exchanges with other behavioral sciences. The early mission of behavioral geneticists centered about the demonstration that genetic variation was an important determinant of most (all?) domains of behavior and, in large part, this goal seems to have been accomplished. Contemporary investigation is much more likely to be concerned with the mechanisms or pathways involved in genetic determination and the attempt to arrive at quantitative estimates of the magnitude of genetic influence under stated conditions. The literature we have reviewed provides clear evidence of the growing influence of both molecular biology and biometrical developments upon research in this area.

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AUDITION¹

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This chapter continues the tradition of selectivity begun by Green & Henning (56) with their review of hearing in Volume 20 of these *Annual Reviews*. Rather than survey all the papers on hearing to appear within some 12-month interval—if, indeed, that were possible—I decided to summarize (in about 25 pages!) what auditory electrophysiologists have done during the last several years.

As I was to learn, even this proved too ambitious an undertaking. As the search through the literature progressed, the field to be reviewed was defined again and again. What remains of the bibliography—which is less than what was ablated—is a list of papers dealing with microelectrode recordings from single mammalian neurons. Excluded from this review are studies of auditory coding in birds, reptiles, amphibians, fishes.

Responses evoked by sound and recorded through larger electrodes are also ignored. These include cochlear potentials, compound action potentials, integrated multiple-unit recordings, and slow-wave responses from auditory centers. Studies of ongoing activity and of the human vertex potential are not reviewed.

The list of references at the end of this chapter includes most of the papers published during the 4-year period from 1966 through 1969. Abstracts, proceedings, unpublished dissertations, and reports that are not generally available have not been cited. I tried to include all the papers dealing with auditory neuronal responses. I apologize for any and all omissions from the list. In all probability, they are oversights.

It seemed reasonable to go back to 1966. Ward's review of hearing (130) for that year is the last one in these volumes to include mention of the electrophysiology of the auditory nervous system. The monograph by Kiang et al (67) appeared in 1965. Since Whitfield's *The Auditory Pathway* (136) was published in 1967, it contains only a few references to papers published during 1966. Reviews by Bishop (12), Schwartzkopf (112), and Goldberg & Lavine (51) in the *Annual Review of Physiology* and the survey by Katsuki (64) all refer mainly to studies that were published before 1966.

The reader will note that there is little mention in this chapter of auditory

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efferents and no discussion of echolocation and the bat's auditory system. These important matters have received excellent treatment in Grinnell's recent survey of comparative auditory physiology (57).

Two symposia of interest were held within 4 months of each other. De Reuck & Knight have edited the Ciba Foundation's volume on *Hearing Mechanisms in Vertebrates* (31); Perkel & Bullock (87) have reported on a work session concerned with neural coding in sensory systems.

The chapter has been organized anatomically, with the final section devoted to theoretical papers. Each of seven major divisions reviews what has been recorded from a particular part of the auditory system. This is not altogether satisfactory. A more functional arrangement was essayed, but the results were even less satisfactory. We are not yet at the point where the events evoked by significant classes of stimuli can be traced through the auditory nervous system.

AUDITORY NERVE

Measurements of auditory-nerve discharges in acute experiments with anesthetized animals may indeed be representative of more long-term measurements involving awake animals. Evidence for this assertion is reported by Simmons & Linehan (115), who were able to record from a single eighth-nerve unit over a 6-week period. With the cat awake or anesthetized, repeated measurements of tuning curves, noise masking, and adaptation all gave consistent results. The animal did show higher rates of firing when awake than when anesthetized. The middle ear muscles were intact. Since cochlear microphonics did show the attenuating effect of the acoustic reflex, there may well be some direct effect of pentobarbital on discharge rate.

Kiang, Sachs & Peake (66) showed that tuning curves for auditory-nerve fibers are less irregular in shape when stimulus levels (plotted on the ordinate) are expressed as amplitudes of stapes displacement rather than as voltages across the earphone or as sound-pressure levels at the eardrum. By removing from the plot the frequency-selectivity of the middle ear (cf. 59, 86), the tuning curves no longer exhibit local minima at frequencies below the characteristic frequency (CF). The curves rise monotonically on both sides of the CF.

Finck (40) recorded from elements in the auditory nerves of hamsters. Response areas were then superimposed on a frequency (30 cells) by intensity (20 cells) grid, and tabulations made of the number of tuning curves passing through each cell of the grid. It was then possible to plot, for each frequency, the intensity level required to activate a certain proportion (e.g. 0.1) of the 164 elements that were studied. These plots resemble audiograms and show, in the case of the hamster, that unit activity is most easily evoked in the frequency region of 9 kHz.

Two-tone interactions are the concern of a number of papers from the Wisconsin and Eaton-Peabody laboratories. The Wisconsin group confirmed in the case of the squirrel monkey what had previously been reported for

the guinea pig, cat and macaque—that auditory-nerve fiber discharges evoked by low-frequency tones are phase-locked to the stimulus waveform (100). For frequencies up to about 5 kHz, interspike intervals are grouped around integral multiples of the stimulus period. If this “period-time code” is actually used for the transmission of low-frequency information, then it becomes worthwhile to examine the synchronization of discharges when two tonal stimuli are presented. With two tones (which are both inside the fiber’s response area), the discharge may be phase-locked to the first tone only, to the second only, or to both (61). Which mode of response obtains is a function of the intensities of the tones and their positions inside the response area.

Whichever is the mode of response, however, the total number of spikes remains essentially the same (101). When the response to two tones is phase-locked to one of them, it is identical in form to that evoked by the “masker” alone. The masker seems to “dominate” the fiber by somehow preventing phase-locked responses to the “masked” tone—not by saturating the fiber with spikes. The neuronal line is not “too busy” (61).

With two tones locked to each other in a frequency ratio of small integers, a complex, periodic stimulus results whose waveform can be defined precisely. “Period histograms,” which show the distribution of spikes throughout the period of such a stimulus, were computed and then compared with the stimulating waveform (i.e. the sum of the component sinusoids). This waveform was “corrected” for the fact that addition inside the ear involves phase angles and relative amplitudes that are different from those in the acoustic waveform (14).

Once this was done, changes in the phase or relative amplitudes of the component tones produced impulse histograms whose form could be accurately predicted from the stimulating waveform. The very close correspondence between the impulse histogram and a half-wave rectified version of the stimulating waveform was taken as evidence that auditory-nerve fibers are excited by unidirectional deflections of the cochlear partition—a suggestion that is, of course, not new.

If temporal spacing of discharges does indeed convey information about low frequencies, and if—for some reason—many interspike intervals in the response evoked by a two-tone complex correspond to neither of the component tones, then the neuron can be said to be reporting on a frequency that was not actually contained in the acoustic stimulus. That this does occur was demonstrated by Rose et al (102). Interspike intervals unrelated to the component tones arise as a consequence of the unidirectional detection (rectification) process in the cochlea whenever the excitation peaks of the stimulating waveform are unequally spaced in time. Interval distributions were found which correspond to the difference tone [high (h)—low (l)]. No correlate was found for the second-order difference tone (21—h), although such should exist under certain circumstances (30).

Goldstein & Kiang (52) were able to obtain discharges synchronous with

($21-h$) when the primary tones, 1 and h , were chosen so that ($21-h$) approximated the CF of the fiber. The primary tones could be outside the tuning curve of the fiber. Furthermore, the synchrony could be cancelled by adding a third tone ($f=21-h$) of appropriate amplitude and phase. The nonlinearity responsible for the generation of responses to ($21-h$) is considered by Goldstein & Kiang to be frequency dependent and of odd order, and to operate even at very low levels of intensity.

Sachs & Kiang (111) reported on two-tone inhibition in eighth-nerve fibers. This inhibition can occur in animals whose olivocochlear efferents have been transected (65). Of 310 fibers studied (CF varied between 0.2 and 35 kHz), all showed diminished response to a continuous tone at CF upon addition of a second tone of appropriate frequency and intensity. Using sweep-frequency procedures to define iso-rate contours, the authors were then able to plot areas of response and of inhibition. The principal features of the inhibitory areas were similar for all fibers studied. This homogeneity with respect to inhibition parallels a homogeneity of elements with respect to threshold (65).

More reliable measurements of discharge rate were made by Sachs (110), who used tone bursts rather than swept frequencies. In addition, frequencies higher than 4 kHz were employed to avoid contamination of rate measurements by the effects of phase-locking. A model was then developed relating discharge rate to stimulus parameters. Rate of firing was derived as the sum of a "spontaneous" rate and a "driven" rate. The latter was developed as a weighted sum of the driven responses to the tones presented singly. The "inhibitory multiplier" in the equation is a function of the frequencies and intensities of the two interacting tones.

COCHLEAR NUCLEUS

In 1967, Whitfield (136) commented on the number of microelectrode studies of the cochlear nucleus (CN). It is still the case that studies of single auditory neurons deal predominantly with cells in CN. Relations between afferents and efferents, between inputs and outputs, between excitation and inhibition are all available for analysis in this first brain-stem center. In addition, the CN presents the investigator with the ever-attractive possibility of correlating detailed anatomy with subtleties of function. Finally, contamination of results by anesthesia is undoubtedly less at this level of the CNS than at the midbrain or in the forebrain.

Pfeiffer (89) recorded spontaneous and sound-evoked spikes from the most rostral portion of the anteroventral CN. In this region, incoming fibers from the eighth nerve have prominent nerve terminals (calyces of Held). Although the spikes were recorded extracellularly, it was possible to distinguish individual presynaptic events from postsynaptic potentials.

Spontaneous spike discharges from units in the ventral CN are eliminated immediately after cochlear destruction. Activity in dorsal CN units, on the other hand, persists, even in chronic preparations (70). Since cochlear

destruction abolishes spontaneous discharges in auditory-nerve elements (67), while producing no appreciable histological degeneration in either ventral or dorsal CN, the authors conclude that eighth-nerve discharges are required for the appearance of spontaneous activity in the ventral CN. The spontaneous discharges in dorsal CN must originate in the units themselves or depend on activity in other inputs to the CN.

Since spontaneous firing by ventral CN units is "driven" by auditory-nerve discharges, Molnar & Pfeiffer (80) attempted to describe the former as superpositions of influences from the latter. Auditory-nerve fiber discharges can be approximated by a renewal process model (55). For cells in anteroventral CN, a model was developed in which the CN unit fires each time an input spike occurs. A small number of identical, independent renewal processes were then superposed to generate interval histograms for the CN unit. For units in posteroventral CN, on the other hand, discharge patterns are better described by supposing that each impulse requires the summation of a number of presynaptic spikes. A Markov process was then utilized to model the accumulation of individual input events.

Tuning curves, intensity functions, and iso-rate contours were described for elements in CN of the white rat (77). These are similar to those described for the cat, except that the highest frequency that could elicit a response was 34 kHz.

Goldberg & Greenwood (50) examined unit responses to stimuli of long duration (10 sec). For tonal stimuli at CF, some units showed a monotonic relation between firing rate and stimulus intensity. For other units, the intensity function first increased to some maximum, then decreased. The two kinds of neurons also differed in their modes of adaptation. Although units differed considerably in their interval distributions, for any single neuron, relations among such summary statistics as mean interval, standard deviation of intervals, and correlation between adjacent intervals were not affected as frequency, intensity, or duration of stimulus was varied.

When narrow bands of noise (centered at CF) were employed as stimuli, the interval histograms showed more relative variability—reflecting, presumably, the statistics of the stimulus. The correlations between adjacent intervals showed little change from the negative values obtained with tonal stimuli. The correlation is probably related to recovery time—an inherent property of the neuron which is more or less independent of the type of stimulus employed.

Mikaelian (76) described two types of units in CN of the mouse. More frequently occurring were those with lower thresholds, narrower tuning curves, and a dynamic intensity range of only 30 dB. Units with broader tuning curves had higher thresholds and a dynamic range of 40–50 dB. Two-tone inhibition could always be shown with units of the first type; it was a rare occurrence in neurons of the second variety.

Pfeiffer (90) essayed a more elaborate classification of CN units—one based on post-stimulus time (PST) histograms. Profiles of the histograms,

which were computed from responses to tone bursts, were arranged into four groups. Moreover, the nerve cells in each group could be localized in specific regions of the dorsal and ventral CN. Three of the four types of units have since been seen in the guinea pig's CN with electrical stimulation of auditory-nerve fibers (91).

Radionova's classification (96) of CN cells was based on differences in response latency. Short latency elements are more variable in their momentary thresholds than are long latency units.

Thus, we have analyses based on intensity functions (50, 77), tuning curves (76), PST histograms (90), and response latencies (96). It remains to be seen whether these different classifications can all be reconciled. Most encouraging are Møller's confirmation (77) of the earlier finding [by Goldberg & Greenwood (50)] of monotonic and nonmonotonic intensity functions and his substantiation of Pfeiffer's description (90) of units that respond only to onsets.

Although Møller (79) proposed that these onset-responding units play a role in mediating periodicity pitch and described units that fired once per click up to rates of 600 clicks/sec, Glatfke (47) was unable to find "envelope following" by low-frequency cells ($CF < 800$ Hz). The animal was stimulated by a 4-kHz sinusoid, which was chopped at frequencies from 0.1 to 1.0 kHz and filtered to exclude energy at the modulating frequency. For high-frequency units, the carrier frequency was set to equal CF. These units responded to the amplitude-modulated tone with discharges that were synchronized with the modulation. Cells in CN are not sensitive to periodicity per se. Once a unit is driven to respond (by providing energy at its CF), the discharge pattern can reflect the fact of amplitude modulation. At the level of CN, however, information necessary to perceive periodicity pitch is not carried by low-frequency neurons.

Responses to frequency-modulated (linearly swept) tones were analyzed by Møller (78). Histograms were plotted of spike counts as a function of tonal frequency. At frequencies of modulation around 20 Hz, the histograms became markedly leptokurtic—reflecting, very probably, temporal properties of the units' excitatory and inhibitory response areas (cf. Grinnell 57, p. 572).

Radionova (95) described the reciprocity (at neuronal "threshold") of intensity and duration for stimuli that were rectangular bursts of noise. Threshold intensity increased by about 10 dB as duration was decreased from 100 msec to about 10 msec. For shorter durations, the trading relation was approximately 30 dB per log unit of time (98). The slope of the log I vs log t plot is steeper if the criterion of neuronal response is 5 or 10 spikes per noise burst rather than one spike—a result that must surely be related to refractory periods and recovery times.

Radionova (93) has also catalogued the effects of changing signal intensity and signal duration on PST and interval histograms. Bursts of tone and of noise were used. The effect of stimulus intensity on spike latency was described and related to changes in threshold with rise-time (94).

Changes in firing patterns (both excitatory and inhibitory) have been studied as functions of tonal frequency and duration by Radionova (97) and by Gerstein, Butler & Erulkar (35, 46). The latter studies are noteworthy for their use of intracellular electrodes in an effort to relate firing patterns to membrane potentials. Discharges evoked by a tone burst were accompanied by membrane depolarizations that lasted for the duration of the burst. Changes in firing rate, however, were not always accompanied by changes in level of depolarization. In addition, suppression of spontaneous spikes and "pauses" in firing during and immediately after stimulation were not accompanied by hyperpolarizations. Inhibitory influences on the CN neurons were presumably taking place at locations remote from the intracellular electrode.

With frequency-modulated tones, changes in firing and in membrane potential were again correlated only weakly. Differences in firing (at some tonal frequency) between positive and negative values of df/dt or between large and small values of the derivative (cf. 78) can be interpreted in terms of excitatory and inhibitory influences on the CN neuron. Unfortunately, the changes in membrane potential that can be recorded provide no substantiation for the models.

Firing by cells in CN is obviously affected by (inhibitory) efferents to the cochlea. In addition, however, there are direct inputs to CN from several efferent bundles. Comis & Whitfield (19) dealt with two such efferent pathways to CN. One, from the ipsilateral S-segment (lateral nucleus) of the superior olivary complex, had only excitatory effects on units in ventral CN. The other, from the contralateral nucleus of the lateral lemniscus, could produce either inhibition or facilitation of discharges.

In addition, the olivo-cochlear bundle sends collaterals into CN (99). Stimulation of the bundle at its point of decussation increased spontaneous discharges in some CN units and decreased firing in others (121). The fact that this modification of spontaneous activity persisted after destruction of the cochlea argues very strongly for a direct efferent effect.

Peripheral activation of one or another of these efferent inputs to CN is undoubtedly responsible for the findings of Pfalz (88), Pirsig & Pfalz (92), and Klinke et al (68, 69). In the first of these studies, spontaneous firings in the left CN were recorded after destruction of the left cochlea (cf. 70). In some cells, these discharges could be inhibited and in others, facilitated by trains of clicks delivered to the right ear.

In the second study, Pirsig & Pfalz (92) utilized their technique of evoking spikes in a CN unit by electrically stimulating basal turn fibers in the spiral lamina (91). These discharges in CN could then be inhibited by tonal stimuli delivered to the contralateral ear.

In the third study (68, 69), dichotic stimulation was employed at levels low enough to minimize cross-conduction. Of 45 neurons in CN stimulated by bursts of tone at CF, 28 showed inhibition (latency: 30–50 msec) upon delivery of a second tone to the contralateral ear. Inhibition was greatest with the second tone slightly above or slightly below CF.

SUPERIOR OLIVARY COMPLEX

Afferent fibers from the cochlear nuclei connect to the superior olivary complex. Of the nuclei and cell groups that comprise this complex, the medial superior olivary nucleus (MSO, accessory nucleus) and the lateral nucleus (*S*-segment) continue to be the principal centers of interest on the part of experimenters armed with microelectrodes. We recall that each MSO receives afferents from both CN, whereas each *S*-segment has only ipsilateral inputs from CN.

Patterns of firing in response to stimulation of one ear with tone bursts were catalogued by Clark & Dunlop (18). The PST histogram was used to describe probabilities of firing before, during, and after the tone bursts. Sixteen types of response patterns were noted and analyzed and their frequencies of occurrence in MSO, *S*-segment and the nucleus of the trapezoid body were tabulated.

Unit responses were also recorded from the nucleus of the trapezoid body to electrical stimulation of the auditory nerve (17). Electrical stimulation at a low frequency failed, in general, to produce the same firing patterns as those evoked by tones of the same frequency.

MEDIAL SUPERIOR OLIVARY NUCLEUS

Rupert et al (107) described unit responses to dichotic clicks as functions of interaural intensity and of interaural time. Neuronal latencies were measured from N_1 responses, so that interaural time differences (Δt) are really interaural differences in N_1 latency. This was done to remove peripheral (intensity-to-time) effects from the measurements of MSO responses.

Under these circumstances, three kinds of units were found in MSO. The first kind responded with one spike to a click delivered only to the ipsilateral ear. Latency of this spike and probability of response are both affected when stimulation is dichotic. The contralateral click has inhibitory effects.

Double-responding neurons fired twice in response to each ipsilateral click. Again, the effect of the contralateral click was inhibitory. Firing of either or both spikes could be suppressed, although the second impulse was the more easily inhibited. For both kinds of neurons, small changes in Δt (about 1.0 msec) could evoke the whole range of inhibitory interactions.

The third type of cell showed binaural interactions with large values of Δt (approximately 10 msec). These cells were always fired by a click at either ear. With dichotic stimulation, the firing occurs predominantly to the click at the leading ear.

Watanabe et al (131) have confirmed the brief time course of inhibition for units that respond only to clicks delivered to one ear and the longer-lasting inhibitory effects in cells that are fired by clicks at either ear. It is not clear from their paper, however, that these were units in MSO.

With coincident N_1 responses ($\Delta t = 0$), the spike response to a click-pair

can be completely inhibited (81). Indeed, inhibition (from the contralateral input) can be seen even when the ipsilateral N_1 leads by 0.6 msec—evidence that the crossed conduction, which involves a greater distance, is faster. Additional evidence for this comes from MSO neurons that fire in response to a click delivered to either ear; contralateral latency is the same or shorter than ipsilateral.

With low-frequency tones (at CF) presented dichotically, inhibition was a function of interaural phase. The range of phase angles over which inhibition occurs is narrowed if the inhibiting tone is diminished in intensity with respect to the excitatory tone.

Goldberg & Brown (48) described unit responses in MSO of the dog. In this animal, MSO is folded into a U-shape; the hilus of the U opens laterally. Cells are arranged tonotopically: units with high CF are in the ventral limb of the U; units with low CF are in the dorsal section. For a given unit, CF is the same when stimulation is contralateral as it is when the tone is delivered to the ipsilateral ear (131). In addition to cells which show inhibitory interactions, there are cells which are excited by both ears and whose firing to binaural stimuli is augmented.

These two types of cells were further analyzed in a companion study (49). High-frequency neurons which show summation or facilitation have steep intensity functions when both ears are stimulated. They are, therefore, much more sensitive to variations of binaural intensity than to interaural differences in intensity. High-frequency cells which show inhibitory interactions, on the other hand, are more sensitive to interaural differences.

Low-frequency units that could be fired by either ear were stimulated dichotically at CF. Firing rate was then a function of interaural phase. As an example, minimal firing (maximum inhibition) to a 444.5-Hz tone delivered to both ears occurred with the contralateral tone delayed by 0.4 msec (64°). This value agrees fairly well with that given by Moushegian et al (81, p. 1256) for a low-frequency neuron that could be excited only ipsilaterally. It represents the time-shift required to set the phase-locked discharges from the two ears to be 180° out of phase. Maximal firing occurred at a phase angle such that the inputs to MSO were in phase. At this point, the binaural responses are phase-locked with high precision—impulse histograms are sharply peaked.

LATERAL SUPERIOR OLIVARY NUCLEUS

Much of what we know about unit responses of the S-segment comes from four papers by Tsuchitani & Boudreau (13, 124–126). Since most S-segment neurons can be fired only by ipsilateral stimulation—they receive direct afferents only from ipsilateral CN—we are not surprised to find that the first two reports by Tsuchitani & Boudreau (124, 125) deal simply with coding of monaurally presented stimuli. Indeed, the authors first considered the S-segment to be a relay station (124).

Cells in the *S*-segment are not spontaneously active (18, 124). They are arranged tonotopically: those with high CF are in the ventromedial limb of the *S*, while low frequency elements are in the dorsolateral section. Most units responded with sustained discharges to continuously presented tones. Intensity functions (for tones at CF) increased monotonically (124); the dynamic range of the functions, however, was often less than 30 dB (125). Generally, units with similar CF had thresholds that varied by less than 10 dB.

Although units in the *S*-segment are excited only by ipsilateral sounds, they can be inhibited by contralateral stimulation (42). Boudreau & Tsuchitani (13) report that this inhibition is long lasting. Its latency is about the same as the latency of excitation and the CF for inhibition is close to the CF for ipsilateral excitation.

In "intensity-trading" experiments, 40-msec tone bursts were presented dichotically, and the intensity increments that produced equal and opposite effects were measured for each ear. The results showed that firing could be "turned on" or "turned off" by increasing tonal intensity at the appropriate ear. In general, excitatory stimulation was more effective than inhibitory. Thus, an increment of more than 1 dB in contralateral intensity was required to offset a 1-dB increase in ipsilateral intensity.

If discharges from *S*-segment cells are governed by differences in interaural intensity of sustained tones, sounds to one side of the head should excite the ipsilateral *S*-segment and inhibit the contralateral nucleus. Although the inhibitory pathway to each *S*-segment remains to be described, it is interesting to note that no inhibitory interactions were found for cells whose CF was below 1.0 kHz. Intensity differences cannot serve as cues for localization of long wavelength tones. The view that the *S*-segment processes binaural intensity differences while the MSO handles time differences has been put forth by Masterton & Diamond (75).

That this picture of the *S*-segment is too simple has been pointed out by Tsuchitani & Boudreau (126) in their most recent paper. We recall the limited dynamic range of most *S*-segment cells and that similarly tuned elements have roughly equal thresholds. This means that binaural intensity differences (with a tone at CF) cannot control neuronal firing precisely when tonal intensity exceeds 30 dB re threshold. At higher levels, the responses of cells which are not tuned to the stimulus must be considered. The higher the stimulus level, the greater is the difference in frequency between the CFs of the most active cells and the stimulus. The locus of maximal firing within the *S*-segment shifts away from cells tuned to the stimulus.

INFERIOR COLLICULUS

Microelectrode studies of the inferior colliculus have continued along the lines laid down by Rose and his co-workers in 1963 (62, 103). Responses to monaural and to binaural stimulation continues to be catalogued and analyzed.

RESPONSES TO MONAURAL STIMULATION

David et al (21-23) have further described firing patterns of collicular units. Intensity functions and PST histograms are presented (22). The former are noteworthy for their limited dynamic range (20 dB and less); changes in the form of the latter (with changing intensity, e.g.) confirm what has previously been reported (103). Phase-locking is seen of discharges evoked by low frequency tones (23).

Joint-interval histograms were used to display various departures from a Poisson process of firing patterns evoked by stimulation (21). Particular interspike intervals and combinations of adjacent intervals were found to be accentuated by certain stimuli and obliterated by others.

Several papers have dealt with unit responses to amplitude-modulated stimuli. Vartanyan (128) related neuronal following of modulation to temporal integration and resolution. Al'tman (9) examined collicular responses to 2-msec noise bursts presented at rates of 20-30 per second. The neuronal threshold was lower for a train of bursts than for a single burst or for sustained noise.

Nelson et al (83) have reported on patterns of unit response to amplitude-modulated and frequency-modulated tones. Attempts to relate these patterns to simple interactions between areas of response and of inhibition were only moderately successful. The authors suggest that an alternate form of "tuning curve"—one based on spike counts evoked by supra-threshold tones of the same intensity—be used to predict unit behavior with respect to modulated stimuli. They note, in addition, that excitation and inhibition must have different latencies if their algebraic addition is to account for responses to time-varying stimuli.

Al'tman (5) has described a curious pattern of discharges evoked by clicks presented in slow succession. In the case of a neuron that was not spontaneously active, clicks presented at a rate of one per 5 sec evoked impulses throughout alternate intervals. The results of stimulation were, alternately, spikes for 5 sec and near silence. The animal, it should be noted, was anesthetized. The alternation of response could be seen for an hour.

RESPONSES TO BINAURAL STIMULATION

Neurons whose firing (by tones) depends upon interaural phase were described by Rose et al (104). Stimulated by different frequencies, firing turns out to be maximal at some value of Δt —at different values of phase angle, therefore—so that a collicular cell can be said to have a "characteristic delay." Stimulation of each ear evokes periodic events that are both excitatory and inhibitory. Interactions among these events can be seen in the case of a neuron which could be excited by tones presented to either ear. With dichotic stimulation—and depending on Δt —the firings could exceed the sum of the two monaural discharges or be absent altogether. The results of binaural interaction also depend on intensity, which presumably determines the magnitudes of the periodic excitation and inhibition.

These effects were seen with stimulating frequencies below 3 kHz. "Characteristic delays" are also seen when the dichotic stimuli are bursts of noise (45). The delays are not appreciably changed even when wide variations in passband and in interaural intensity (± 20 dB) are introduced. The results with both tones and noise can be modeled by supposing that a collicular neuron computes the cross-correlation (with delay equal to its "characteristic delay") of the inputs to the two ears. (The inputs are, of course, filtered before they are multiplied.)

Neurons which are sensitive to binaural differences in tonal intensity have both high and low CFs (104). In general, contralateral stimulation is excitatory, and interaural intensity differences of only a few dB are reflected in changed rates of firing. This effect of interaural intensity is independent of overall level with tonal stimulation; it is level-dependent when noise bursts are used (45).

With click-pairs serving as stimuli, changing the intensity of one member of the pair alters the mean number of evoked spikes (7). Although mean firings are again changed by small variations of binaural intensity (approximately 2 dB), there is serious trial-by-trial variability resulting from the fact that only one or two impulses are fired on each trial.

"Are there neurons detecting direction of sound source motion?" Al'tman's reply is, "Yes" (8). Dichotic clicks were presented at rates of 8–30 pairs per second. Within each train of clicks, Δt decreased, then increased. (This corresponds to moving the click source from the leading side toward the midline, then back.) Although this modulation of Δt was symmetric, ten cells were found in which the response was asymmetric: firing rate at some value of Δt was enhanced or inhibited, depending on the direction of change.

MEDIAL GENICULATE BODY

"Of all the major cell stations of the auditory system, the medial geniculate body has been the least studied electrophysiologically." This observation by Whitfield (136, p. 99) continues to be true; only ten papers were found which deal with neurons in the pars principalis of the medial geniculate body. (Responses from the magnocellular nucleus are considered below in the section on centers outside the auditory system.) Eight of the papers were published in 1966 or 1969. All ten reports, we note, came from laboratories outside the USA.

Animals were anesthetized in all but one of the studies. Only three papers report results obtained from animals immobilized with gallamine triethiodide.

Tuning curves for geniculate units were plotted and classified by Maruyama et al (74). Phase-locking of responses is seen, but only for an extremely narrow range of frequencies (23). Many units show an early "onset" response which is separated by a pause in firing from later discharges (22, 74). This firing pattern, we recall, is manifested by many col-

lular neurons and by units in CN. Other units show initial inhibition (34).

Units that are fired by tones usually discharge only in response to contralateral stimulation (1). Binaural interactions are facilitatory for some neurons, inhibitory for others. Inhibition is almost always ipsilateral in origin and is maximal when the dichotic tones are of the same frequency.

Responses to clicks are also excitatory or inhibitory. In addition, some neurons exhibit reverberating responses in which periods of excitation and suppression alternate for several hundred milliseconds (4). Excitation, inhibition, and reverberation can also be seen in geniculate discharges evoked by electrical stimulation of the brachium of the inferior colliculus (3).

Units of the first (excitatory) type were subjected to further study (2). Inhibition following the evoked discharge could be seen in the depressing effects of repetitive stimulation at rates of about 20 clicks/sec and in the diminished discharge to the second of two closely spaced clicks. The latter effect was also observed with dichotic clicks. Here again, inhibition by a first click can occur even when that click does not fire the geniculate cell.

Now if the inhibiting click is delayed so that it follows the exciting click, firing of the geniculate cell should increase sharply. This is precisely what happens (6).

In a few geniculate units, sound-evoked firing can be affected by prior electrical stimulation of the ipsilateral auditory cortex (132). In general, shocks delivered to cortical area AI were inhibitory in effect while stimulation of AII was facilitatory. The authors rule out the possibility of antidromic effects by noting that the latency of inhibition is long (8–30 msec). They note, too, that similar effects of cortical stimulation can be seen in the inferior colliculi. The effect at the midbrain is bilateral and of longer latency.

Aitkin & Dunlop (3), however, found only short latency (3–5 msec) suppressions of thalamic responses with stimulation of cortical AI. They consider the short latency (though long lasting) suppression to result from antidromic activation of geniculate cells.

AUDITORY CORTEX

Katsuki (64) and Evans (36) have summarized prior studies of cortical units from the Tokyo and Birmingham laboratories. Organization of the cat's ectosylvian cortex into vertical columns is discussed by both authors and by Rose (36, pp. 289–90), who confirms that all cells within a column tend to have similar CFs. The columns are narrow, however, and the functional organization within a column remains obscure. Evans further emphasizes the failure of the Birmingham group to find tonotopic organization of the auditory cortex at the single neuron level.

Goldstein et al (53, 54) have catalogued the responses of units in cortical area AI to a "standard" set of simple stimuli (tone bursts, noise bursts, clicks). Experimental animals were immobilized with gallamine triethiodide and the stimuli were presented to both ears simultaneously, to one ear, or the other ear.

Almost all of the units studied responded to at least one of the three standard stimuli by increased firing, decreased firing, or a combination of the two. With bursts of tone or of noise, responses occurred at onset, at termination, or throughout. Combinations of these patterns were also seen. A few units responded only to special, "nonstandard" stimuli such as swept tones. Presented with a standard array of stimuli, the 118 units that were studied showed little lability of response and "performed in a highly individualistic and variegated manner" (54, p. 444).

Considerable lability of response, on the other hand, was mentioned by Whitfield (136, p. 123). Vardapetyan (127) has detailed the changing responses of AI units in animals anesthetized with chloralose. Tone bursts and noise bursts were presented at rates of about one per 3 sec. Of 72 units studied, 11 showed responses that decremented with repetition of the stimulus bursts, 10 showed incrementing responses, and 16 were simply unstable. The author reports that firing that had been "extinguished" would reappear if one or another parameter of the stimulus bursts was changed.

Firing is diminished in response to the second of two closely spaced bursts of tone or of noise (85). Recovery of firing requires approximately 100 msec. Nonmonotonic recovery functions resemble those seen with geniculate neurons (2) and remind us of the two-click studies of gross evoked potentials by Chang (16) and by Rosenzweig & Rosenblith (106).

Binaural representation and interaction in cortical units was described by Hall & Goldstein (53, 60). With the "standard" stimuli (mentioned above) presented monaurally and binaurally, it was found that most units were influenced by both ears. There was extensive overlap of the populations of units representing the two ears; the population representing the contralateral ear is larger and includes most of the population representing the ipsilateral ear. Again, we recall a similar conclusion from studies employing gross cortical electrodes (cf. 105). Binaural occlusion, inhibition, and summation were observed. The last—wherein the binaural response is stronger than the response to monaural stimulation of either ear—was the most commonly seen form of interaction.

Binaural interactions with tonal stimuli were further described by Brugge et al (15). One group of cortical cells was excited by contralateral stimulation and inhibited by ipsilateral stimulation. Although changing interaural intensity affected the number of firings markedly, latency (to the first spike) was only little affected. The data were interpreted by considering that latency of (contralateral) excitation and latency of (ipsilateral) inhibition are similar functions of tonal intensity.

For neurons that respond to stimulation of either ear, sequences of excitation and inhibition from the two ears are presumed to interact. The outcome of the interaction would depend on the relative timing of the events from the two ears. These, in turn, are functions of intensity.

Finally, for frequencies below 2.4 kHz, neurons were found which were

sensitive to interaural phase. These cells have "characteristic delays" and behave in much the same manner as those described by Rose et al (104) in the inferior colliculus. Brugge et al (15) consider that these cells are cortical recipients of discharges from cells in lower centers of binaural convergence.

NEURONAL RESPONSES IN OTHER CENTERS

Unit responses to sound have also been recorded outside of the principal (afferent) auditory tracts and nuclei. Cells in several cortical areas and in a number of subcortical centers have been explored.

"NON-AUDITORY" CORTEX

Gross et al (58) recorded responses to clicks and to tones from the superior temporal association cortex of monkeys. Both anesthetized and immobilized animals were used and gave identical results.

Spontaneous firing by units in the visual cortex can be increased or decreased by tonal stimulation (118). In most neurons studied, the firing rate was decreased. Repeated presentations of the stimulus tone "extinguished" the inhibitory response. Flash-evoked responses in the visual cortex were also inhibited by sound (117). Recordings with intracellular electrodes showed that this inhibition was not accompanied by hyperpolarization (116).

A number of other studies have described cortical cells that could be fired by stimulation of more than one modality. This "convergence of inputs" has been seen in the visual cortex (10, 119) and in the motor cortex (129).

"Polysensory" cells have also been noted in several association areas of the cortex. Auditory, visual, and somatosensory stimuli are all able to evoke firings in units of the middle suprasylvian gyrus (11, 32, 82, 122, 129) and in units of the anterior marginal gyrus (109). Cells in the insular cortex can be fired by clicks and by flashes (72).

SUBCORTICAL STRUCTURES

Feldman & Dafny (37) recorded responses to clicks from units in the posteromedial hypothalamus. In most cases, the effect of stimulation was to increase firing rate over spontaneous levels. Polysensory cells could be excited by auditory and by somatosensory stimulation.

Polysensory cells are also seen in the main sensory trigeminal nucleus (33), in the magnocellular division of the medial geniculate body (73, 135), in the superior colliculus (63), and in the lateral geniculate body (71). The units in the superior colliculus showed diminished firing after repeated stimulation; responsivity returned after a lapse or after some parameter of stimulation was changed. In the lateral geniculate body, the principal effect of sound on flash-evoked impulses was facilitatory. Since this differs marked-

ly from the inhibitory interaction described for cortical neurons (cf. 117), the cortical effect cannot be ascribed to thalamic interactions.

Activation by sound of centrifugal neurons was described for both auditory efferents (108) and visual efferents (120). The auditory elements were isolated in the medulla and were functionally similar to those previously contacted with a microelectrode in the internal auditory meatus (38, 39). The visual efferents, which were in the optic nerve, responded to sound and not to light. Click stimuli were able to modify visual afferent discharges (120) and visual receptive fields (133).

O'Keefe & Bouma (84) reported on single unit firing in the amygdala. Electrodes were implanted; the cats were unrestrained. Several units were found which responded selectively to such complex sounds as bird chirps, human speech, hand claps. After-discharges lasting 10 sec and longer were seen.

Firing patterns which varied according to conditioning procedures were described by Travis & Sparks (123). Electrode assemblies were implanted in the brains of squirrel monkeys. Two tones were used; one was followed by shock, the other by food. Differentiated response patterns for the "positive" and "negative" tones were seen from a number of neurons in such structures as anterior commissure, corona radiata, globus pallidus, cingulate and sensorimotor cortex. No such effects were noted in two control animals for whom food and shock were omitted.

THEORETICAL PAPERS

A number of theoretical papers appeared during the 4-year period under review. Although the papers vary considerably in their character, each can be assigned to one or the other of two groups. In one group are statistical formulations which attempt to model the discharges (spontaneous and evoked) of first-order auditory neurons. The other group includes papers in which firing patterns (and their fluctuations and transformations) are related to such psychophysical phenomena as masking, intensity discrimination, frequency discrimination, spatial localization.

A theoretical paper cannot be condensed into a few sentences. Therefore, the paragraphs that follow tell what each development is *about*, rather than what it *is*.

MODELS OF THE PERIPHERAL AUDITORY SYSTEM

Weiss (134) has described a three-stage model of the ear-plus-auditory nerve. A linear mechanical component transforms sound-pressure waveforms into displacements of the cochlear partition. A nonlinear "transducer" relates the displacement at a point along the length of the partition to the output of a hair cell located at that point. A "neuron" completes the model. Here the filtered output of the transducer is mixed with Gaussian noise to yield a fluctuating "membrane potential." If the potential exceeds some "threshold" value, a spike is fired. The threshold is then reset to a

large value from which it recovers. The noise makes the model probabilistic in character. In addition, properties of refractoriness and threshold are obviously present. Although the effects of efferents are ignored and no inhibitory mechanism is included, we note that spontaneous firings can occur.

Since firings (spontaneous or evoked) occur when the fluctuating membrane potential exceeds a threshold, the problem of computing firing patterns reduces to the problem of computing distributions of axis crossings. This is difficult to do; the model was, therefore, simulated on a digital computer.

Repetitive firing evoked by stimuli of long duration was further considered by Geisler & Goldberg (44). The random component of the membrane potential was made adjustable in power and was added to a dc "excitatory drive" which was also adjustable. The threshold function included an absolute refractory period of 0.7 msec and a relative refractory period whose time-constant was the third principal parameter of the model.

With low-frequency tones, the "excitable drive" was made by Geisler (43) to include the previous dc component and a sinusoid whose frequency equalled the tonal frequency. Phase-locked responses were then generated by the model, and interval histograms were found to resemble those described by Rose et al (100).

In the models developed by Weiss and by Geisler, firing probabilities are based on two mechanisms: stimulus-produced excitation and time since firing (refractoriness). Since these two processes interact in complex fashion in the generation of PST histograms and interval histograms, they cannot be separated easily in the usual analyses of spike data. Gray (55) has dealt with this problem of separating the two mechanisms by removing (controlling) the effects of refractoriness. By estimating conditional probabilities (i.e. probabilities of firing in an interval given that the last firing occurred in a specified earlier interval), stimulus-related effects in the spike data could be studied separately.

When the time since the previous firing is sufficiently long (20–30 msec), the conditional probabilities become independent of this time. This special set of conditional probabilities is called "recovered probabilities." The latter are free of refractory effects and can be related to stimulus variables through such plots as "recovered probability histograms."

Cross-correlational methods were employed by de Boer and his co-workers (24–29) in analyses of the peripheral auditory system. The input to the system is, of course, an acoustic signal; the output is the train of spikes in an auditory-nerve neuron. If the input is white noise, the cross-correlation function between input and output is the average waveform of the noise in the vicinity of the spike. The portion of the correlation function that is obtained by advancing the spike (delaying the noise) is now the average waveform of the noise just prior to the elicitation of a spike.

A tape recorder can delay the noise appropriately, and an average response computer can be triggered by the spikes to perform the computations (25, 27, 28). Since the averager is triggered by the output of the

auditory system, rather than by its input, de Boer calls this process "reverse correlation." In addition, since the correlation is not between two continuous signals, but between one signal and trigger pulses derived from it (by the process of excitation), the computation is termed "triggered correlation," as distinct from "true correlation" (25, 29).

For a linear system driven by white noise, the cross-correlation function between input and output is the same as the system's impulse response. The "impulse responses" obtained in actual experiments on auditory-nerve units show considerable "ringing." Tuning is sharp for eighth-nerve units, even when the input to the ear is noise (24, 26, 28). Since experimental impulse functions are "triggered" rather than "true" correlograms, they include an error term (25, 29). The effects of nonlinearities on sharpness of tuning are mentioned by Davis and by Zwislocki (26, p. 555).

AUDITORY CODING

The microelectrode has made it possible for us to detail how one auditory neuron responds to an ensemble of stimuli. What we really want to know, of course, is how one auditory stimulus is represented in an ensemble of neurons. The problem of auditory coding continues to be vexing.

Whitfield devoted the last quarter of his monograph (136) to chapters on frequency, intensity, and localization. Two additional papers (137, 138) summarize his notions on these matters; a third (139) deals with responses to frequency-modulated tones.

Mention should be made, finally, of Siebert's discussions of coding in the auditory nerve (113, 114). Crozier (20) long ago pointed out that we discriminate between stimuli by means of the neural effects they evoke. The "noisy" nature of this coding of stimuli sets limits on the detection and discrimination performance of the organism.

A modern era in the study of these matters began when Fitzhugh (41) took pains to analyze the variability of firing by retinal ganglion cells. "Physiological difference limens" were then generated by comparing unit responses in the presence of a background illumination with those evoked by the addition of an increment flash.

Siebert's approach represents a significant improvement over the procedures employed by Fitzhugh. The sampling error inherent in the generation of discrimination data from the discharges in one cell is appreciably diminished in Siebert's use of a "whole nerve." This "auditory nerve" was "reconstructed" by combining unit records from many animals (67). Distributions of the total spike output of the nerve were approximated for stimuli such as tone-bursts and noise-bursts. Statistical decision theory was then used to determine the smallest differences between two stimuli that could be reliably discriminated by an "ideal computer" operating on the spike output of the nerve. The performance of this computer constitutes a bound on the sensitivity of real listeners.

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SPATIAL VISION

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INTRODUCTION

Classically, visual perception has always been regarded as having two aspects: the perception of space and the perception of things. These two phrases are, in fact, chapter headings in William James' *Principles of Psychology*. However, there has never been any general agreement as to what constitutes spatial perception and in what way it is to be distinguished from form perception. The general views which underlie the material presented in this chapter may be briefly outlined. We believe that there are two fairly distinct aspects of spatial perception which have not generally been distinguished from one another. One aspect concerns the spatial constancy of the visual environment, and the other, the spatial qualities or attributes of form or pattern perception. The spatial constancy of the visual environment refers to the stability of objects in the face of constant movements of the eyes, head, and body. Objects retain their relative orientations and locations both with respect to one another and with respect to gravity despite constantly roving eyes and changes of head and body posture. The topographical organization within the visual system is oculocentric so that spatial constancy can be achieved only by taking into account the relative positions of the two eyes, both with respect to one another and to the head, on the one hand, and the orientation of the head with respect to gravity, on the other. The concept of spatial constancy has obvious links with what may be called the constancy of spatial distinctness. Because of foveal specialization, visual acuity declines rapidly with eccentricity away from the fixation point. Nevertheless, all the various features of quite large objects seem equally distinct even though the appearance of the object has been constructed from a series of inspections of the different parts made in rapid succession. By contrast with the first aspect, the spatial attributes of form perception relate to the spatial order of the contour features of individual objects on the one hand and the orientations and spatial relationships between the various objects in the field of view on the other. Most of the monocular cues to depth such as object overlay and texture density gradients are among the spatial attributes of form or pattern. These spatial attributes are relational qualities that subsist between the parts of the one object and between the various parts of different objects. They belong in the category of higher-order stimulus variables as discussed by Hochberg (82a). Spatial

constancy phenomena, on the other hand, are more concerned with the visual field as a whole and its relation to information derived from the other senses.

Stereopsis makes a contribution to spatial perception which is additional to and quite distinct from the two aspects we have just considered. The neural mechanisms underlying stereopsis apparently operate at an early stage in the chain of events leading up to pattern recognition. The effective stimuli for stereopsis are apparently local contour features of a fairly simple kind quite unlike the higher-order stimulus variables such as texture density gradients.

A strong case can be made for the view that the search for underlying neural mechanisms is most likely to be profitable in respect to those aspects of visual perception for which there exists a well-developed body of psychophysical knowledge. On this basis, the distinction between the various aspects of spatial perception is a matter of some importance since it means that a satisfactory account of the neural mechanisms responsible both for the spatial constancy of the visual environment and for some aspects at least of stereopsis are likely to become available in the not-too-distant future. The progress that has been made in respect to these two matters will, in fact, be our main concern in the present chapter. Unfortunately, we cannot entertain the same optimism with respect to the spatial attributes of pattern perception. The problem of form perception continues to be particularly intractable, and only the first steps have so far been made towards a psychophysics of space based on the components of pattern discrimination. We have chosen to consider this aspect of spatial perception as falling outside the scope of the present chapter.

Since our account is largely concerned with neural mechanisms, we consider the title *Spatial Vision*, as proposed by the Editors, as more appropriate than *Spatial Perception*. Spatial vision was last reviewed for *Annual Review of Psychology* by Boynton (25) in 1962. While the present chapter covers much of the material that has become available over the past 10 years or so, we concentrate mainly on contributions made over the last 3 or 4 years. A particularly valuable account of human spatial orientation, covering material up to 1966, has been provided by Howard & Templeton (86).

MONOCULAR SPATIAL VISION: SENSORY ASPECTS

Over the past 2 or 3 years there has been renewed interest in the idea that the localization of an object in space involves a neural mechanism that is separate from that used to identify it as a distinct form (75, 76, 98, 155, 156, 180). Ablation of neural centers in animals (97, 156, 162, 179) indicates that in mammals the visual cortex is the probable site concerned with identification, while localization appears to depend on the functional integrity of the superior colliculus and, to a lesser extent, the pretectum of the midbrain. Evidence of a similar dual mechanism in the human is at

present available only from psychophysical experiments, although there have been occasional reports of patients with cortical blindness, following destruction of the striate cortex, who have been able to detect moving objects in the scotomatous areas of their visual field (4, 45).

Held (76) has endeavored to show that localization and identification can be separated in human spatial vision. When three collinear dots are held 10° to the side of a fixation point, they do not appear to be aligned, but the top and bottom dots appear laterally displaced. In contrast, a line appears "straight," suggesting that line stimuli are processed in a manner that differs from the dot stimuli, which have little contour information. Also, Held (75) claims that this functional division can be seen in the adaptation to the distortion introduced by prisms since there is a rapid adjustment of visually guided movements but no change in the distorted appearance of patterns. Thus, after some weeks of wearing half prisms, Kohler (114) found that an object "kept its position" as he looked first through the prism and then through the plain part of the spectacles, in spite of the discontinuity of contour that still persisted.

Trevarthen (180) has pointed out that the accuracy of localization is fairly uniformly distributed throughout the visual field but that foveal vision is far superior to peripheral vision for the identification of objects (cf. also 66). Trevarthen has also remarked on the correlation between these functional abilities and the distribution of rods and cones in the retina, and Held (75) has noted that adaptation to disturbed localization occurs in scotopic vision when detailed pattern vision is poor. However, a strict relationship between localization and scotopic vision on the one hand and identification and photopic vision on the other is unlikely because it would obviously result in poor localization in bright light when foveal vision is being used. Held (75, 76) has outlined the respective contributions that the localizing and identifying systems are likely to make to spatial vision. The form discriminating or identifying system could extract properties that are invariant over a wide range of sizes, orientations, luminances, colors, and even distortions. In this process relational and nonmetrical properties are more important than metrical ones. On the other hand, the locating system would be concerned with the metrical properties of space, which are important for the purposes of manipulating objects and for moving about the environment.

THE NATURE OF THE RETINOTOPIC PROJECTION

Classically the topographical projection in the visual system has been regarded as providing the neural basis for spatial vision. While there is undoubtedly a fairly precise point-to-point projection from the retina to the visual cortex, relatively little is yet known as to why the neural mechanisms for spatial vision should require the preservation, at the level of the cortex, of the same spatial relationships between neural elements as exist in the retina. One important aspect of the retinotopic projection is that it provides

for the development of functional cell columns in the lateral geniculate nucleus (12) and in the visual cortex (88, 88a).

Until recently, the evidence for the identity of cell columns extending across the thickness of the cortex from surface to white matter has been of a physiological nature, but Hubel & Wiesel (93) have now provided an anatomical demonstration of columns in the monkey striate cortex. The columns provide for the segregation of cells which not only have an input from a very localized area of the retina but are also concerned with a specific aspect of visual information coming from that area. Thus, some of the columns are concerned with receptive field orientation and others with eye dominance. A cell column can be looked upon as representing a direction in the visual field, the cells being aggregated so as to facilitate the application of similar neural data-processing steps to all the cells in the column. The retinotopic projection also provides the basis for the orderly conjunction of the fiber pathways from corresponding retinal areas in the two eyes onto the cells in the striate cortex. The binocular innervation of striate cells in this way probably forms the basis of the neural mechanism for binocular depth discrimination (see below). Doty (47), however, has indicated certain difficulties with the topological approach to the explanation of visual phenomena.

Only with the development of single cell recording techniques has it been possible to estimate the precision of the topographical projection. Hubel & Wiesel (88, 91) have shown that the detailed topographical representation does not hold at the microscopic level. As a microelectrode is advanced down a column of cells perpendicular to the surface of the striate cortex, there is no net change in the position of the receptive field of successively recorded cells. There is, however, an irregular, apparently random variation in receptive field position from cell to cell, and Hubel & Wiesel regard this random scatter as a measure of the degree to which the connections fall short of a precise point-to-point projection from the retina. In the monkey striate cortex, the recording of cells along the length of a column showed that the random variations in receptive field position covered an area two to four times that of the average receptive field size (91). Taking the average receptive field for foveal or parafoveal vision to be 0.5° across, this represents a range of 1.5° for receptive field centers. The comparable estimate for the cat striate cortex is probably slightly larger, possibly 2° – 3° . A rather similar estimate has been made of the scatter of receptive field positions for cell columns in the representation of the area centralis in the cat lateral geniculate nucleus (152), taking care to restrict consideration to cell columns that lie along projection lines (12). Cells lie along a projection line if there is no systematic drift in receptive field position from one end of the column to the other.

By way of a quick estimate of the precision of the point-to-point projection to the cortex, Hubel & Wiesel (92) have formulated a rule of thumb which states that the random scatter in receptive field position is roughly

the same as that of the area of the largest receptive field found in an electrode track. The smallest receptive fields are found for cells in the geniculocortical pathway that receive an input from the central retina (100, 194). Cells in the higher order visual centers in the brain might be expected to have larger receptive fields than the individual lower order neurons supplying them, but this does not seem to be the case. Cells in areas 17, 18, and 19 have receptive fields that are similar in size to those found in the lateral geniculate nucleus. The point-to-point projection to each of these three cortical representations of the contralateral visual hemifield, therefore, attains much the same level of precision [cf. (200) for monkey]. Since the analysis of form continues into areas 18 and 19 (89), it is hard to avoid the conclusion that the point-to-point projection is an essential component in the neural mechanism for form discrimination. Exact isomorphism does not seem to occur anywhere along the visual pathways, and indeed, Bishop (7) has pointed out that there may be a functional advantage in the random variation in the receptive field positions in the case of cells in the striate cortex. It could provide the structural basis for the distribution of receptive field disparities required for the binocular depth discrimination mechanism.

It is a little surprising that the point-to-point representation of the retina is as precise as it appears to be along the geniculocortical pathway. Electron microscope studies have shown that on each relay cell between receptors and cortex there are secondary synapses coming from neurons off the straight through pathway. In the retina, horizontal cells and amacrine cells bring signals from an extended region of surrounding retina (49, 184), and in the lateral geniculate nucleus, nonretinal synapses on the relay cell come from cortical neurons either directly or indirectly via the axon of an interlaminar cell (67, 68, 70). Synapses from intracortical axons are also present on cortical cells (32). In spite of these complex synaptic patterns, one-to-one pathways through the retina are possible (24, 115), and the size of the center of the receptive fields of cells in the lateral geniculate nucleus approach the diameter of a single retinal receptor (194). One-to-one straight through pathways also seem likely in the lateral geniculate nucleus of the cat (10).

In the subprimates, disruption of the retinal projection in the striate cortex has little observable effect on pattern vision. In these animals, however, there are, in addition, well-ordered independent topographical projections from the retina through the thalamus to other cortical centers, and for pattern vision to be lost, these centers have to be destroyed as well (162). Recent work has confirmed that destruction of the striate cortex in primates leads to a loss of pattern vision (38), and it is perhaps significant that it is in the geniculocortical pathway that the point-to-point projection from the retina reaches its highest level of precision.

In the cat there are at least two retinotopic projections to secondary neural centers that are off the main retino-cerebral pathway, namely to the suprasylvian gyrus in the cortex and to the superior colliculus in the mid-brain. Each is now known to receive a similar dual afferent nerve supply with

one input coming from the retina (via the lateral geniculate in the case of the suprasylvian gyrus) and the other coming from the visual areas of the cortex (59-61). In the superficial layers of the superior colliculus, the topographical projection from the retina is in register with the projection back from the visual cortex so that axons from areas 17 and 18, related to a particular part of the retina, pass to that part of the superior colliculus which receives fibers directly from the same site in the retina (59). Little is known about the retinotopic accuracy of this link at the level of the single cell, nor is its functional significance understood. In some species the deep layers of the superior colliculus are known to receive a proprioceptive input from the extraocular muscles (190), and these connections could be of importance for visual localization. In the primate there is no cortical region exactly analogous to the visual area in the cat suprasylvian gyrus since the geniculo-cortical projection in these animals goes solely to area 17 (195). An extra-striate area in the monkey, the inferior convexity of the temporal lobe, makes a contribution to detailed pattern recognition (130), but apart from gross histologic measurements (117), nothing is known concerning the precision of retinotopic projection to this area. Receptive fields in the superior colliculus and the suprasylvian gyrus in the cat are much larger (up to 30° or more) (48, 92, 173) than those in the main visual centers so that the retinotopic projection, therefore, is less precise. This has prompted the suggestion that the suprasylvian gyrus, like the superior colliculus, may assist in the localization that underlies visually guided behavior (48).

Speculation continues on the function of lamination in structures like the lateral geniculate nucleus and the superior colliculus. In the normal lateral geniculate nucleus, the representation of the contralateral hemifield for each eye lies in register in neighboring laminae. Recently it has been shown that the laminar pattern is disrupted in the Siamese cat (69). It seems reasonable to link these findings with the common occurrence of binocular imbalance in these animals and to suggest that the orderly topographical projection to the lateral geniculate nucleus functions in the interests of binocular vision since it allows cells receiving inputs from corresponding areas in the two retinas to be in close apposition.

There is no obvious extension of the topographical projection onto the motor side of the visual system. While the patterns of eye movements occurring after electrical stimulation of the occipital cortex have roughly the directional characteristics that might be expected from the sensory retinotopic projection, there can be no certainty that electrical stimulation of the occipital lobe is not simply an indirect way of activating sensory mechanisms (51, 182). In the motor regions of the frontal lobe, topographical order is not apparent (51) and the fairly strict isomorphism of the sensory pathways is lost in the frontal eye fields.

THE CHARACTER OF SINGLE CELL RECEPTIVE FIELDS

Almost everything known about the receptive fields of single cells in the geniculo-cortical pathway (including cortical areas 17, 18, and 19) is com-

patible with belief that the cells are concerned with pattern recognition (80, 88, 89). With the discovery that the superior colliculus has a special role in spatial localization, there has been a renewed interest in the character of collicular receptive fields. In the cat, collicular cell receptive fields are similar in some respects to the complex and hypercomplex receptive fields of the visual cortex, but they differ in being much larger, less sensitive to change in the orientation of contours, and strongly preferring movement away from the midline of the visual field (171, 173). According to Wickelgren & Sterling (191), collicular cells no longer show a preference for stimuli of a particular direction of movement when the visual cortex is inactivated by cooling. If this is the case, the information about form that comes from the cortex is of a rather imprecise nature. However, others have been unable to demonstrate this cortical dependence (84, 96, 123). The unique centrifugal direction selectivity of the receptive fields of collicular cells is probably organized within the colliculus itself, but to sustain this response it might be necessary for information about the direction of movement of the stimulus to be available from the cortex (120). Collicular cells seem more concerned with the movement of objects rather than their shape. Receptive fields of cells in the suprasylvian gyrus of the cat have also been examined and compared with those in the superior colliculus (92). The larger size of the receptive field seems the main point of difference between suprasylvian cells and those in the visual cortex, and they do not share the peculiar direction selective characteristics of the collicular cells (92). The selective nature of the response by suprasylvian cells to stimuli of different orientation suggests that some information about visual form is of importance for the function of this area.

BEHAVIORAL DISTURBANCES FOLLOWING NEURAL ABLATION

Two major problems beset experimenters using the results of ablation of neural centers as a guide to the normal functioning of the visual system. Firstly, there is the difficulty of confining the lesion to the neural center being studied and then there is the problem of isolating the resulting behavioral deficiency. To assist in making a decision about behavioral deficiencies in respect to spatial vision Trevarthen (180) has drawn what appears to be a reasonable association between certain movements on the one hand and locating and identifying vision on the other. Postural adjustments and general movements in space are provided for by locating vision, whereas fine manipulative acts directed towards specific objects fall within the province of identifying vision. The idea of two such visual mechanisms has been discussed above and while it has much to commend it, Trevarthen is nevertheless rather too ready to fit all the observations into what must necessarily be an oversimplified scheme. Everything that he considers not directly concerned with form vision is linked with locating vision and thus color vision and brightness discrimination are associated with the midbrain (180) despite the fact that all the evidence in the primate indicates that color discriminations, at least, are made in the geniculo-cortical system (194).

Cortical ablation.—There is considerable species variation in the behavioral effects resulting from cortical ablation. In the cat there are at least four different retinotopic projections to the cortex, and contralateral hemianopia occurs only when the whole of the occipital and part of the temporal cortex is removed from one hemisphere (162). In the golden hamster, there is a failure to discriminate visual patterns when the striate cortex is ablated, but the animal is nearly normal in its ability to localize objects in space (156). Fine pattern discrimination is also lost in the monkey following striate cortex ablation (38), but recently Humphrey & Weiskrantz (97) have shown that moving objects can be visually located and accurately grasped (cf. also 141).

Early ablation experiments seemed to deny that areas 18 and 19 have a role in the recognition of spatial patterns (cf. also 144). The insignificant behavioral disturbance that follows ablation of portions of the occipital lobe in the cat make the interpretation of ablation experiments difficult in this animal, but even in the monkey, Lashley (118) had difficulty in finding a deficiency in pattern recognition after ablating areas 18 and 19. Zeki (199) re-examined Lashley's results and discovered indications of deficits in the retention of preoperatively learned pattern recognition. It is informative to read Zeki's critical analysis of Lashley's work, for his comments have an important bearing on investigations of this type. He believes that preoperative overtraining and incomplete ablations were responsible for minimizing the deficit in Lashley's animals, resulting in a positive finding being interpreted as an apparently negative one. It is unquestionably a field where differing experimental procedures make comparisons between results difficult. Recently Zeki's (198) own ingenious experiments on split brain monkeys showed that if the circumstriate area (areas 18 and 19) in one hemisphere is destroyed while the other is kept as a control, the animal learns to recognize patterns more slowly through the eye projecting to the damaged hemisphere. Beyond area 18 and 19 in the monkey the inferior convexity of the temporal cortex makes a contribution to pattern recognition, but Mishkin (130), in a series of cross-lesion experiments, has shown that it can only do so provided a large area of the circumstriate cortex is intact. The contribution of the temporal cortex seems to depend on neural processing occurring in areas 18 and 19. So far, in the monkey all results indicate that the cortical areas concerned with vision are involved in pattern recognition. This seems to be the case also in man, but in addition the large parietal cortex makes a very significant contribution to highly specialized pattern recognition such as reading the printed word (39, 40). In the monkey, lesions of the inferotemporal cortex resulted in an apparent difficulty in remembering stimulus-reward relationships, and there seems to be a deficit in associative learning (99, 196).

Collicular ablations.—The results of ablating the superior colliculus in primates are disappointing for those anticipating dramatic effects as a result of a loss of locating vision. Anderson & Symmes (1) found only slight visual

deficiencies after bilateral ablation of the superior colliculus in macaque monkeys, although when the lesions invaded the pretectum and the posterior medial pole of the thalamus, extensive retraining in learning both color and pattern tasks was necessary. Denny-Brown (44), after aspiration of one superior colliculus, found monkeys tended to circle towards the side of the lesion, but in general the deficit is now thought to be a slight one. In an excellent survey of the effects of colliculectomy in cats, Sprague & Meikle (165) describe neglect of the contralateral hemifield following unilateral ablation, while stimuli in the ipsilateral hemifield evoke a heightened response so that frequently there is a tendency for the animal to circle towards the side of the lesion. Sprague & Meikle (165) produce convincing evidence that loss of the tectum alone will cause this syndrome but that the pretectum may also play a part in the integration of visually guided behavior. Bilateral superior collicular ablations result in initial blindness in the cat, with permanent deficits in visual following and localizing (50), although, $2\frac{1}{2}$ months after the operation, a cat can jump with good judgment from a 3-foot-high table to the floor (165). In the golden hamster (156), defects in visually guided behavior follow bilateral ablation. The animals were unable to go from starting position to goal under visual guidance, but they still showed themselves capable of fine pattern discrimination.

Some recent observations apparently contradict the theory that the superior colliculus does not participate in the identification of patterns. Sprague and his collaborators (163, 164) found that cats with chiasma and forebrain commissures split and with unilateral destruction of the superior colliculus (including the deepest layers) and the caudal part of the pretectum had marked deficits in pattern discrimination when tested through the eye on the affected side but behaved normally when tested through the other eye.

Destriate and colliculectomized animals.—A number of authors using ablation techniques have demonstrated a functional link between the visual cortex and the superior colliculus. In the monkey a small lesion in the striate cortex combined with a colliculectomy results in far more visual impairment, with poor recognition of objects, than occurs from isolated ablations in either visual region alone (1). However, a much more systematic series of lesion experiments using crossed ablations, with more rigid controls and more specific details of behavioral deficit, are required for the primate. Sprague's (162) experiments on the cat have pointed the way. Hemianopia was first established by removing one occipito-temporal cortex. Then subsequent removal of the contralateral superior colliculus or section of the collicular commissure restored vision to the hemianopic field. Visually initiated movements were used to define the limits of the visual fields. Apparently both cortex and colliculus are capable of initiating visually guided behavior in the cat, but the colliculus on one side can only take over from a damaged ipsilateral cortex if released from the inhibition coming from the colliculus of the

other side. Unfortunately, because of the extensive ablation in the cortex, the particular cortical region responsible for mediating visually guided behavior has not been isolated. Experimental evidence in favor of the two regions—colliculus and visual cortex—having equivalent functions has yet to be produced for any animal other than the cat.

Some of the uncertainty in interpreting the results from ablation experiments in the primate arises because of the functional pre-eminence of cortical structures whose removal so visually incapacitates the animal that it is difficult to ascertain the functional contribution of the remaining sub-cortical process. Isolating the part played by the superior colliculus in the animal with cortical ablation is further complicated by mutual inhibitory influences passing between one superior colliculus and its partner on the opposite side of the midbrain.

Split brain experiments.—The results of experiments in monkeys where all the forebrain commissures and the optic chiasma have been divided indicate that locus-specific information about visual space is transferred from one hemisphere to the other at the level of the midbrain while the more detailed information on pattern discrimination crosses in the forebrain commissures (180). These results are of importance for they provide the first direct evidence of a physiological nature for a dual mechanism (i.e. locating and identifying) underlying visual perception in the primate. First, Trevarthen (179, 180) showed that the degree of interhemispheric transfer depends upon the nature of the visual stimulus. Thus, if an animal sees one circle with one half of his split brain and another circle with the other half, he can distinguish that the two are different in size but he cannot distinguish two stimuli that differ in figural content. Using an ingenious experimental procedure, Trevarthen (178, 180) taught split brain animals to select one member of a stimulus pair with one eye and the opposite member of the pair with the other eye so that when both eyes were used the two eyes had mutually contradictory answers to the problem. When the two members of the paired stimulus contain figural information (a cross and a circle) one or other of the responses is always adopted, and the lack of confusion in the monkey's behavior suggests that interhemispheric transfer does not occur. In contrast, with two nonpatterned stimuli differing only in color or brightness, Trevarthen (180) presents evidence of interhemispheric transfer, but Hamilton & Gazzaniga (72), using a different procedure, could not find any indication of midbrain transfer of color, luminosity, or pattern information (cf. also 73). The site at which color and luminosity information is transferred, therefore, is a little uncertain, but in the cat, Meikle (126) has shown luminosity discrimination need not be transferred through the corpus callosum. It would not be surprising if color discrimination, a cortical achievement, is found to be transferred through the cortical commissures. The experiments of Bossom & Hamilton (20) and Hamilton (71) add weight to the idea of a dual visual system, one for locating and the other for identifying. They

found normal transfer of visually guided hand movements in split brain monkeys, and they also found that adaptation to displaced localization resulting from a prism before one eye is completely transferred to the other hemisphere (cf. 64, 65).

There are indications in the primate that interhemispheric transfer of visual pattern information is not limited entirely to the cortical commissures. In the split brain monkey, Gavalas & Sperry (62) found that two pairs of half circles projected to each hemisphere combined interhemispherically to form a full circle (the correct choice) and a half circle in visual space. In the absence of interhemispheric transfer, only half circles would be seen, and since the split brain monkey was able to make the correct choice, this type of pattern discrimination must be transferred interhemispherically in the absence of forebrain commissures.

DEPRIVATION EXPERIMENTS

A short period of monocular deprivation by lid closure between the fourth and eighth weeks of a kitten's life leads to a lowering of visual acuity and a reduction in form vision (46). Hubel & Wiesel (94) attribute this loss to the disruption of highly specialized connections onto neurons in the visual cortex, for although no histological changes can be detected (193), there is almost complete disappearance of cells that can be driven by stimuli exposed to the eye that had been closed. Transmission of signals through the lateral geniculate nucleus seems normal although, strangely enough, there are signs of histological change in the lateral geniculate nucleus (192). Ganz et al (58) have also reported a reduction in the amplitude of the *b* wave in the ERG. Hirsch & Spinelli (82) claim that a highly selective disruption of cortical connections can be caused by deprivation because cats raised from birth in an environment consisting of only horizontal and vertical lines were found to have cortical cells with only horizontally or vertically oriented receptive fields. Unfortunately, the moving spot of light that they used for mapping the receptive field is a relatively insensitive method for defining its optimal orientation. Dews & Wiesel (46) were unable to demonstrate a reduction in a cat's ability to place its paw accurately on a pier on the other side of a water barrier even though deprivation had resulted in a quite severe loss of form vision. However, many other reports have shown fairly conclusively that there are at least transient defects in visually guided behavior when the animal is deprived of visual information of limb movement by rearing him with a ruff around his neck to prevent him seeing his legs (74, 77). Unlike the normally raised animal, the ruff-reared animal, when lowered over a horizontal grid, failed in placing his paws on the bars. Also, Held & Hein (78) found that animals passively moved around in an environment that would in other respects allow the development of normal pattern vision were defective in making visually guided movements. A nice experimental control is added by showing that the ability of the two eyes to control movement can be dissociated by closing one eye during passive movement and the other eye

during active movement (75). No such dissociation between the eyes when performing figure discriminations is found if the testing procedures do not make use of visually guided responses (129). Flexion of the hind limb was used as a response in this experiment because it was felt that in the past a loss of form discrimination has been mistakenly reported because a visually guided act has been used as a test for the defect. The transient nature of the defects in visually guided behavior reported by Hein & Held (74) are quite different from the permanent loss of pattern vision described by Dews & Wiesel (46), suggesting that different neural pathways are affected by deprivation.

The results from ablation and deprivation experiments indicate that localization and identification are the products of different neural processes, but it now seems unlikely that the processes involved in localization are limited exclusively to the superior colliculus or that those of identification occur only in the cortex. Thus, the distinction between the two systems is not as clear cut as one may have been led to believe from the earlier accounts.

MONOCULAR SPATIAL VISION: MOTOR ASPECTS

The contribution of the motor side of the nervous system to the final assessment of visual space depends upon the system's ability to appreciate and control the position of the eye. Two neural reactions are involved in keeping the image of an object steadily positioned on the fovea. The first mechanism uses a corrective type of eye movement based on retinal image feedback to restore fixation and hence to center the oculocentric field. On the other hand, the second mechanism simply holds the eye steady with respect to gravity despite head and body movements. It is of less importance in man and acts on information from the vestibular system to hold the visual field steady rather than correct for a displacement after it has occurred. While signals from the retina can be used to seek out and hold the image of a point on the fovea, they do not permit an assessment of the spatial location of this point relative to the whole organism, and a neural appreciation of the position of the eyes and of the relationship between the visual axis and the body axes is necessary before oculocentric field locations can be converted into egocentric spatial directions (183). Although the basic requirements for an eye position sensing mechanism were established by Helmholtz (79), it is only in the last 2 or 3 years that a start has been made to discover the precise nature of the underlying neural mechanisms.

CENTERING THE OCULOCENTRIC FIELD

The important studies of Rashbass & Westheimer (146-148), have confirmed the division of visually directed eye movements into three categories: saccade, smooth pursuit, and vergence. Each movement has such distinctive properties that the writers suggested that each was controlled by a separate neural subsystem. Recently, Robinson (149, 150) has presented two excellent reviews on the control systems of eye movements.

The saccadic system.—A single control system seems to be responsible for both the microsaccades (or flicks) of fixation and the voluntary saccades of searching and tracking. Each type of saccadic movement has a similar rate of acceleration (52, 201) and similar intersaccadic intervals while reading and fixating (43). Both are directed towards making rapid corrections between desired and actual directions of the visual axis (23). It has long been realized that a state of relative refractoriness exists in the saccadic system during its 200 msec central reaction time (146, 150, 188), but recently surprisingly short intersaccadic intervals have been found to follow saccades greater than 15° (3). Becker & Fuchs (3) suggest that this small intersaccadic interval facilitates the centering of peripheral images, but they regard it as a "pre-package" double saccade of an otherwise normal type rather than the product of a separate control system. Large tracking saccades are clearly under voluntary control and microsaccades can also be voluntarily suppressed (169). The motor eye field in the frontal lobe (Brodmann area 8), described in classical literature as the voluntary eye center (83), has been advanced as the cortical motor center responsible for initiating saccadic movement (151). However, the classical accounts describe the eye movements resulting from stimulation of this area as slow rotations of both eyes to the opposite side with an occasional up and down component (42). Recently, Robinson & Fuchs (151) found that electrical stimulation of the frontal eye field (Brodmann area 8) in the unanesthetized monkey always produced saccadic eye movements, and the slow movements of the classical experiments are attributed to the effects of anesthesia. Smooth pursuit movements are lost in light barbiturate anesthesia while the animal is still active, but as the animal becomes unconscious, saccadic movements cannot be evoked and are replaced by the slow movements reported in the classical experiments (151). A similar loss of saccadic movement is observed in sleep (57).

The saccades induced by electrical stimulation are all-or-nothing responses but, rather surprisingly, when frontal eye fields in both hemispheres are stimulated simultaneously, the response may be graded, possibly because of interaction in the midbrain. Even the contribution of the frontal eye field is uncertain, however, and bilateral ablation of area 8 in the monkey fails to produce an observable functional deficit (138). Pasik & Pasik (138) concluded that no single cortical center is responsible for initiating a specific type of eye movement and suggested that oculomotor function is widely dispersed over the entire cortex (cf. also 51). The results from Bizzi's (14, 15) single cell recordings in the frontal eye field also seem to rule it out as a region for initiating saccadic eye movements. If the cells of the frontal eye field are not the originators of oculomotor signals, the work of Robinson & Fuchs (151) suggests they must surely be intimately linked with the cells that are responsible.

The smooth pursuit system.—For the fovea to follow a moving retinal image, the smooth pursuit system constantly samples and responds to the

velocity of a moving target (146), although recently Pucket & Steinmann (145) have revived the possibility that the stimulus for smooth pursuit is the rate at which the eye fails to match the movement of the target. The slow drifts that occur during fixation have been dissociated from the smooth pursuit system and attributed to the inherent instability of the oculomotor system (37). St-Cyr & Fender (168) have detected that drifts have a slight centering function, and Steinman et al (169) found that the variability of eye position around its mean fixation point did not increase in those subjects who voluntarily suppressed saccades. Very few people are able voluntarily to suppress or to control a smooth pursuit movement (170, 185) and in the dark, slow drifts lose their centering function during attempts to hold the eye steady (159) so that a visible stimulus is necessary to initiate this involuntary response. The classical center for "automatic" eye movements is the occipital cortex, although the belief has little to support it (41, 150). Much store has been placed on the loss of the slow phase of optokinetic nystagmus following destruction of the striate cortex, but the interruption to the sensory side of the reflex could explain this loss, and even in the monkey the loss is not permanent (138). Apparently at variance with the idea that the occipital cortex is the involuntary motor center, Bizzi (14, 15) found single cells in the frontal eye fields that discharged during smooth pursuit movements, but since the firing seldom preceded the movement, another site of movement initiation must be sought. The susceptibility of the slow phase of optokinetic nystagmus to midbrain ablations has been frequently tested and the results show many species differences. The removal of the superior colliculus in monkeys does not produce a detectable loss (139), and only when the lesion extends into the pretectum is there a deficit in vertical optokinetic nystagmus (140). Walls (183) recognized two different types of optokinetic nystagmus: the first, called "stare" nystagmus, is associated with the superior colliculus and is found in animals whose retina lacks a specialized central area, and the second, called "look" nystagmus, which has developed in addition to stare nystagmus in animals with specialized central areas in the retina. "Look" nystagmus, commonly called the fixation reflex, involves the visual cortex in mammals (50, 97). Since the smooth pursuit movement of optokinetic nystagmus can be initiated both in the midbrain and in the cortex in mammals other than man, it is another indication that there are two neural levels of spatial vision (180).

A great many experiments have been conducted to see how closely the oculomotor system approaches a linear feedback control system when tracking a moving stimulus (53, 167, 168). It is beyond the scope of this review to consider the results of these experiments in detail, but it now seems widely acknowledged that a simple linear model fails to account for all aspects of tracking behavior. The transfer characteristics of the oculomotor system are linear only under very limited conditions of target motion, and St-Cyr & Fender (167) express dissatisfaction with models involving the use of such devices as dead zones, limiters, samplers, and predictors for which there is

often no obvious physiological correlate. To test the linearity of the system, a number of most ingenious optical methods have been evolved to alter the magnitude and sign of retinal-image feedback (53). An extension of these methods has important implications in the interpretation of the results of inverted image experiments (161). Smith (160) claims that adaptation to inversion of the retinal image is impossible if the retinal feedback is also reversed. Inverting spectacles worn on the head or nose do not invert the retinal effects of ocular motion but alter only the feedback effects of head and body motions. Adaptation is possible to these more generalized head and body feedback disorientations because the normal feedback from eye movements helps to compensate, but when eye movement retinal image feedback is also reversed (by dove prism on a contact lens) adaptation is impossible. There seems no doubt that these observations will have important bearing on the theory of perception and learning.

The vergence system.—The independence of conjugate and disjunctive eye movements is well demonstrated by their separate contributions when the eyes move to take up eccentric fixation (150). Binocular disparity, which brings about a vergence movement to restore binocular single vision, is constantly sampled. The disparity appears to be assessed at a cortical level because it cannot be transferred between hemispheres in the human subject with section of the corpus callosum (186). Unlike smooth pursuit movements, the velocity of convergence movement is not directly related to the velocity of target movement but is proportional to retinal image disparity (147). Because disparate configurations with quite dissimilar shapes for the two eyes will evoke a vergence movement, Westheimer & Mitchell (186) suggest that the cortical cells must have large non-shape-specific receptive fields. This would perhaps be out of character for cortical neurons, and it may well be an oversimplification to imagine that a single type of cell is involved in the assessment of large retinal disparity. Apart from this suggestion of cortical involvement, little is known about the central neural pathways responsible for vergence movements although electrical stimulation of the frontal eye field occasionally causes convergence (42). Straschill & Hoffmann (174) have also found cells in the pretectum that fired only when the stimulus approached the eye.

EGOCENTRIC STABILIZATION OF THE OCULOCENTRIC FIELD

The vestibular system is the main factor concerned in the egocentric stabilization of the oculocentric field. Since the vestibular system has been recently reviewed (28), we shall not consider it here. However, there is one paper which is of special interest because it refers to the possibility of changing the properties of cortical receptive fields as a result of alteration of the head and body axis. Horn & Hill (85) reported a change in the angle of orientation of a slit of light that produced the best response from a simple cell in the cat striate cortex when the animal was tilted through angles up

to 50°. This change was nearly always in the direction opposite to the body tilt, apparently to provide a neural compensation for the tilting. Although the authors do not mention it, such a compensation would be additional to the compensatory countertorsional eye movements that occur under natural conditions. Command signals for counter eye torsion may well have been generated in these animals but with unknown central effects. Actual eye torsion was prevented in the Horn & Hill experiments by sewing the eye to a ring. In view of the obvious importance of these results, it is to be hoped that a more detailed account will be presented in the near future. Some criticisms of the preliminary account, however, may be made. In some of their graphs the preferred orientation cannot be defined within about 25°, and the issue is further clouded by the fact that changes of preferred orientation of a magnitude similar to the observed effect occurred spontaneously. Complete compensation for body tilt is not to be expected in any case because of countertorsional eye movements.

NEURAL APPRECIATION OF EYE POSITION

Muscle proprioception.—It has often been pointed out (e.g. 29) that the retina fulfills for the eyes the proprioceptive function that is served elsewhere by the muscle sense. However, besides the visual feedback, a second independent pathway must carry "extra-retinal" information about the eye's position for egocentric directions to be established and for the effects of eye movement to be distinguished from object movement (53). The evidence for the "extra-retinal" path being a proprioceptive one from the extraocular muscles is still unconvincing, although in goats and many other ungulates, nerve signals have been recorded in the trigeminal nerve following stretch of the extraocular muscles (34, 35, 121, 122, 189, 190). This neural path has been traced back to the deeper laminae of the superior colliculus (190). Both muscle spindles and tendon organs have been found in the extraocular muscles of man (33), but muscle spindles have not been found in cat or monkey (33, 35), and there are a number of species where there is no obvious anatomical basis for a muscle proprioceptive mechanism. However, even in the cat and monkey, Cooper & Fillenz (36) recorded afferent signals in the oculomotor nerve following stretch of the inferior rectus muscle, and afferent signals from the extraocular muscles have also been recorded in the cerebellum of the cat (56). In man the muscle spindles are found in the smaller muscle fibers of the outer surface of the muscle where, it has been suggested, they exert a controlling influence on tonic contraction (190).

The position regarding eye muscle proprioception has not changed since Brindley & Merton's (26) precise psychophysical observations in 1960 (cf. also 55). They confirmed that passive changes in eye position went unnoticed when the eye was occluded. When the eye was allowed to see, passive movements produced a shift of the external world which, of course, does not happen with normal vision. Also, after-images were now immobile. The subject is apparently only conscious of his intention to move his eyes (128). There seems a reluctance to discount eye muscle proprioception com-

pletely since subconscious neural loops have not been excluded. The stage at which "eye position" information enters the visual system is often immaterial to the argument since it may not always be necessary to distinguish between muscle proprioception and motor center efference (125). Helmholtz (79) originally ruled out a muscle proprioceptive explanation because it failed to account for the stability of the environment during voluntary eye movements. Both in the efference theory and its later elaboration, the corollary discharge theory (176), eye position information is available before or during the movement rather than at an interval after the occurrence of the movement.

The precision achieved in sensing the position of the eye has been estimated in a number of recent experiments requiring the subjects accurately to direct a visual axis in the dark. Sherrington (157) made observations in this regard, but in 1961 Merton (127), using an after-image technique to measure the error in directing the eye in the dark, found subjects less able to turn their eyes to the correct position than Sherrington had suggested. It now seems that the inaccuracy of locating the eye in the dark is related to large involuntary movements [called a "nearly-random walk" by Matin et al (124)] that occur during the first few seconds in the dark. There is no evidence that the eye position is being sensed during this initial period (125). After a longer period in the dark the eye can be more accurately held in a selected position. Skavenski & Steinman (159) believe that with darkness there is a period of transition during which the oculomotor system switches from a retinal to an "extra-retinal" position control. Following this transition there is an "extra-retinal" signal of considerable fidelity that is not contaminated by apparently unsensed involuntary movements. When saccadic movements are voluntarily suppressed in the dark there is a complete loss of eye position sense, and it seems, although Skavenski & Steinman (159) do not carry the argument quite this far, there is an efficient efference signal from the saccadic system but not from the smooth involuntary system.

Efference mechanism.—At the cortical level in the primate the anatomical link between sensory and motor sides of the visual system appears to be limited to projections from the circumstriate cortex (area 18 and 19) and the inferior temporal cortex on the sensory side to the frontal eye field (area 8) and the other parts of the prefrontal cortex on the motor side. Cells of the prefrontal cortex project back onto the inferior temporal convexity to create a return path of unknown functional significance. There is no projection from the visual sensory cortex to the precentral somatic motor area (area 4), and there is therefore no direct path for visually guided manipulative actions (117). Classically the occipitofrontal projection is described as responsible for activating cells in the frontal eye field (area 8) to initiate visually directed voluntary eye movements. Evidence for this, however, is meager (42), and there is a great need for more information about the cortical destination of neural paths leaving the frontal eye field. [For paths to midbrain see Kuypers et al (116).]

Perhaps the most important single development in the search for the nature of the functional link between sensory and motor sides of the visual system has come with the recent technical achievement of recording from single cells in unanesthetized and unrestrained monkeys. For the first time it has been possible to record the relationship between the firing of a single cell and simultaneously monitored eye movements in the intact animal. The results of Bizzi's (14, 15) single cell recording in the frontal eye field have a number of unexpected features. The cell's discharge always follows rather than precedes the eye movement associated with it. It is only the cells that fire during smooth pursuit eye movements (Type II) that can be discharged antidromically from the cerebral peduncle (and therefore from the oculomotor nuclei). It seems that cells that fire during a saccade (Type I) do not send axons to the midbrain oculomotor nuclei. The input to Type I and Type II cells has not been ascertained; their discharge is unmodified by head movements (16), darkness, and inappropriate eye movements (15), so the input does not appear to come from the visual system, the vestibular system, or muscle proprioceptors. Collaterals from widely dispersed cortical oculomotor neurons (138) could provide the input to cells in the frontal eye field. Although this would explain how electrical stimulation of this area evokes an eye movement, it would not explain why only saccades result from stimulation or why, in natural activity, the discharge is delayed until after the onset of the eye movement. To account for the absence of a direct path from Type I (saccadic) cells to the midbrain oculomotor nuclei and the more efficient position sense present when the saccadic system is operating (159), one may postulate the existence of an extrapyramidal pathway, perhaps via the caudate nucleus (22), for transmitting the corollary discharge to the sensory side of the visual system. Lesions in the frontal eye field would then be expected to cause errors in the judgment of space with consequent difficulties in visuomotor performance (19, 177).

The position of both head and eye are registered by cells in the frontal eye field. Only saccades in a given direction or smooth pursuits at a specific location in the visual field are effective in discharging cells of Types I and II. There is a third group of cells whose firing accompanies head movements. These three cell types acting in concert could lead to the establishment of egocentric directions in space (16).

No sign of corollary discharge associated with eye movements has been detected in the monkey striate cortex. Irrespective of whether the retinal image displacements are due to eye movement or movement of the object, the neural response in the striate cortex appeared to be the same (197). In contrast, the firing of a proportion of cells in the superior colliculus of the cat is directly dependent upon the factor responsible for the retinal image movement. Some cells are insensitive to displacement of the image caused by eye movement, while others fire to any image movement. A third group will even fire to eye movement in the dark (175). There is also some evidence that eye movement causes change in field potentials in the lateral geniculate nucleus

(30, 31, 112). Bizzi (13) observed an increase in spontaneous activity in single geniculate cells during rapid eye movement sleep.

The role of the frontal eye field is not yet resolved, although it appears to be a region more concerned with sensing the eye position than with instigating eye movements. The lateral geniculate nucleus and superior colliculus could also contribute to the neural appreciation of eye position.

STEREOPSIS

Until about a decade ago, research in stereopsis continued along the lines first laid down by the 19th century investigators such as Helmholtz and Hering. This traditional approach, summarized by Ogle (136), concerned itself largely with psychophysical problems of a static geometric nature such as the quantitative relationship between retinal image disparity and perceived depth, the analysis of the horopter, and the critical disparity limits for single vision. The problems posed by this approach have been fairly thoroughly investigated and have resulted in an impressive body of psychophysical knowledge. Within the last decade, however, we have seen a radical new advance in our understanding of binocular single vision and stereopsis, perhaps the two major influences being the introduction of random-dot stereograms by Julesz (101) and the application of the receptive field concept to single cells in the visual cortex (87). There has also been a much greater interest in the dynamics of binocular vision as a pattern-matching process requiring the continuous coordination and adjustment of the extraocular muscles as a result of retinal image feedback.

RANDOM-DOT STEREOGRAMS: LOCAL AND GLOBAL STEREOPSIS

Local stereopsis.—The early work by Julesz using random-dot patterns is now so well known that only a brief summary is needed here (cf. 103). His technique has shown that stereoscopic depth can be perceived in the absence of any monocular cues to depth or monocular form perception and of all binocular depth cues except retinal image disparity. The left and right random-dot targets are identical except that a center square (or another form) is shifted horizontally in one target by an integral number of dots as though it was a separate sheet. Viewed monocularly each of the targets appears to have the same random texture. When viewed stereoscopically the horizontally shifted area is seen in depth either in front of or behind the unshifted area. The binocular correspondence of the square dots or clusters of dots in the two targets is established by the brain without actual recognition of any pattern. The pattern-matching mechanism associates together similar clusters of dots in the two stereograms. It operates on a dot-by-dot or cluster-by-cluster basis, and line contours of any appreciable length are not required. The term "local stereopsis" has been applied to this apparently simple local process (106).

Julesz (102) specifically examined the role of contours in stereopsis. The common definition of a contour as the monocularly seen boundary of a recog-

nizable object does not apply to a random-dot pattern. Contours were defined by Julesz as local maxima of brightness gradients between black and white squares or groups of squares in the monocular field. He studied the importance of both the location and direction of the local gradients in the two stereograms. Stereopsis was abolished if the direction of the brightness gradients across the contours in *one* of the stereo fields was reversed by complementing the black and white squares even though the actual location of the contours between black and white remained the same as in the original stereo field. On the other hand, a uniform shift in the position of the contours by expanding the black squares by 20% in both dimensions with the direction of the gradient held constant, still maintained strong stereopsis. The fact that the direction of brightness changes are more important than their exact location finds an explanation in the receptive field properties of binocularly activated striate neurons (see below).

The observation that a monocular global percept (shape recognition) is not necessary as a preliminary for stereopsis suggests that the underlying neural mechanisms for depth experience precede those responsible for the perception of form. This suggestion is supported by the recent demonstration that monocularly seen shapes may be made to disappear when viewed binocularly (104, 105). Thus a left field of otherwise random dots may contain a written text of black or white letters composed of clusters of dots which can be perceived and read with the one eye. The right field is then derived from the left by alternately shifting every even and odd horizontal stripe of width equal to two dots to the left or right direction respectively. When this stereoscopic pair is presented binocularly to subjects who have not previously seen either field monocularly, the impression is of a transparent uniformly textured surface above a textured background. The subjects are unable to find and read the disguised word even when instructed to search for it.

Global stereopsis.—In his recent work, Julesz (cf. particularly 106) has emphasized the distinction between local and global stereopsis. Random-line stereograms were generated in which the only monocular cues were minute breaks occurring at small irregular intervals along the length of the lines in closely spaced line grids. The lines in the grid were actually composed of small line segments of constant length so that the breaks were always integral numbers of line segments apart. Once again the stereograms presented a uniform texture to monocular view but the breaks in the lines were binocularly correlated. The question of global stereopsis arises in a particularly acute way in the case of these patterns. When there are a large number of identical line segments or picture elements on the same horizontal line for both eyes, which element in the left field will belong to which element in the right? If one considers only the local stereoscopic process operating on the basis of an element-by-element comparison, there are a large number of possible local solutions and the problem of ambiguities can only be resolved

on a global basis. The term "global stereopsis" refers to the binocular pattern-matching process which finds in the left and right fields corresponding large areas composed of local picture elements or line segments having the same disparity values that make up a form such as a center square and its surround. The random-line stereograms reproduced in Figure 1 of Julesz & Spivack (106) actually have alternate global solutions. Since 50% of the picture elements in this particular stereo pair are identical in the left and right fields owing to chance alone, these elements could have been perceived in a depth plane with zero disparity, 25% could be fused in a lace-like transparent plane slightly in front of the zero disparity plane, and the remainder could be perceived in a plane slightly behind the transparent zero disparity plane. The fact that the global solution actually selected involves the binocular perception of form (center square above the background) indicates that the central nervous system tries to align the eyes so as to bring into register the maximum number of image elements that have the same disparity value and are close to each other. Thus the global stereoscopic process apparently favors a solid, densely-packed organization with a large disparity in preference to a transparent lace-like organization even if the latter has much smaller disparity.

One can imagine the operation of local stereoscopic processes at an early level of binocular integration in the central nervous system, making element-by-element comparisons. Various local matches of picture elements will take place as a result of the vergence motions of the two eyes in binocular fixation. At the next hierarchical level a global stereoscopic process might then select the particular set of local matches which lead to the emergence of binocular form. Having discovered the optimal stereoscopic solution, by trial and error as it were, the global process might then attempt to hold binocular fixation so as to preserve the particular set of local matches. Convergence motions are necessary to bring and hold the picture elements within the critical limits of disparity necessary for stereopsis, but thereafter disjunctive eye motions are not necessary. Random-dot stereograms can be perceived with tachistoscopic presentation (102, 103) and under stabilized image conditions (54).

Pulfrich phenomenon.—The use of random-dot patterns has provided direct evidence in support of the classical explanation of the Pulfrich phenomenon, namely, that the filter has the effect of delaying the arrival of information from the eye with the filter. Julesz & White (107) used a computer-generated film loop in which each frame consisted of two random-dot stereo fields placed side by side. The presentation of one member of each stereo pair was, however, delayed by locating it in the next later frame so that the patterns were displayed sequentially to each eye. The random-dot texture, however, differed for each successive field presented to the one eye—only the binocular shape (global organization) remained the same. A stereoscopic film projection technique using polarizing filters ensured that the right and left matrices of each frame were presented separately to the right and left eyes.

A neutral density filter could be chosen for the eye receiving the "leading" matrix such that the observer was able to report stereoscopic depth. At 12 frames per second and a luminance of 0.1 foot lambert, optimal depth perception was obtained with a filter of 1.5 log unit density. The filter had the effect of bringing the presentation of successive frames within the temporal disparity limits for stereopsis. An estimate of the temporal disparity limit for a luminance of 0.1 foot lambert can be obtained from the fact that the lowest film speed at which no filter was required for stereopsis was about 24 frames per second. Taking into account the discontinuous motion of the film, one frame would succeed another after an interval of about 11 msec. At 19 frames per second (interval 13 msec) stereopsis was difficult to obtain. While such estimates depend to some extent upon the luminance used, these disparity limits are very much shorter than those obtained by Ogle (137) using vertical rods (see below).

In the Pulfrich phenomenon a time delay is interpreted by the nervous system as a spatial displacement of the object. Another example of the translation of a time delay into a spatial displacement is given by Smith's recent study (160) of the effects of retinal image movement related to eye movements. He examined the regulation of the direction and speed of eye movements in relation to the direction and speed of movements of retinal images, and the disorganization of visual perception that results from delays and inversions in eye movement-retinal image feedback system. It is interesting that a delay in retinal image feedback was experienced by the subject as space displacement.

FINE AND COARSE STEREOPSIS: DISPARITY LIMITS

Under conditions where retinal image disparities provide the only cues, it is still possible to localize visual stimuli correctly even though they are presented with disparities so large that they may appear double. Since there is no general agreement as to what constitutes stereoscopic perception, it is hardly surprising that there is uncertainty about the maximum disparity limits for stereopsis. Based on estimates of the disparity limits and the quality of the depth experience, there seem to be two kinds of stereopsis that have yet to be clearly distinguished from one another. For the purposes of discussion here these will be referred to as fine and coarse stereopsis respectively. Such a distinction is of some importance because the suggestion will be made below that separate neural mechanisms may be involved.

Fine stereopsis.—The first of these two categories of stereopsis has been studied in detail by Ogle (135, 136) and subdivided by him into patent and qualitative stereopsis respectively. Using a thin vertical needle in conjunction with a central fixation point, and considering here only central vision, Ogle (135) reported the range of disparities as being ± 15 min of arc for patent stereopsis, with a qualitative appearance of depth extending a further ± 5 min of arc. Another estimate of what is presumably fine stereopsis has

been provided by Fender & Julesz (54) using binocularly stabilized retinal images, although the authors were primarily concerned with the fusional process. Binocular stabilization enabled the left and right images of a stereo pair to be slowly and symmetrically pulled apart on the two retinas after the images had first been brought into alignment and fused. The use of binocular stabilization had the advantage that it eliminated the disparity effects that would ordinarily have occurred under normal viewing conditions as a result of corrective eye movements. We can compare observations that they made with vertical line stimuli on the one hand and random-dot stereograms on the other. With continued separation of the two vertical line images, the break from fusion was sudden and well defined at a disparity of about 65 min of arc. After the break from fusion, however, the two lines were not perceived in depth, presumably because of the absence of a reference plane under the conditions of the experiment. The minimal stimulus-conditions for stereopsis (Panum's limiting case) requires that two lines be presented to one eye and one line to the other eye. When the random-dot stereo images were slowly pulled apart, stereopsis and fusion were not lost until the disparity reached 2°. In order to re-establish fusion, however, considerable reduction in the disparity was necessary both with line stimuli and the random-dot targets. Vertical lines were not fused again unless the disparity was reduced to about 42 min of arc. In the case of random-dot targets, fusion and stereopsis were not reestablished until the disparity had fallen to 6 min of arc.

Fender & Julesz (54) emphasize that random-dot targets are of particular interest on two counts: 1. Since there is no monocular perception of shape, it means that stereopsis cannot exist in the presence of double vision. 2. Random-dot targets provide much more binocular disparity information than do line stimuli so that much larger disparities can be tolerated before stereopsis is lost. The much greater effectiveness of random-dot patterns in respect to stereopsis is presumably due to the fact that they are densely covered with picture elements each carrying disparity information whereas line stimuli are relatively devoid of disparity cues. All the above evidence suggests that for central vision the neural mechanisms concerned in fine stereopsis cannot be set in train unless the disparities concerned are quite small.

Coarse stereopsis.—There are many indications that depth discriminations of a rather coarser kind can be made with disparity limits much greater than those we have just discussed. Thus, with momentary presentation of point targets seen in diplopia, Westheimer & Tanzman (187) reported that judgments can be made with disparities up to 7° that are significantly better than chance as to whether the test points are "in front of" or "behind" the plane containing the fixation point. However, even at 1° disparity, the smallest disparity tested, the responses were only 80-90% correct.

Another indication of the disparity limits for depth discrimination can be obtained by studying the disjunctive or vergence eye movement responses

by which adjustments are made for differences in fore and aft positions of visual targets. Using vertical line stimuli, Westheimer & Mitchell (186) showed that the horizontal disparity limits for the initiation of vergence eye movements lay between 5° and 10° . When presented with a 2.75° horizontal disparity, targets so dissimilar as to be incapable of fusion (e.g. circle in one eye, cross in the other) also elicited vergence movements indistinguishable from those stimulated by vertical lines. The direction of the movement was always correct no matter whether the targets were similar or dissimilar. The vergence movement, however, did not proceed to completion presumably because images as dissimilar as those mentioned cannot be fused. Since the degree of similarity of the disparate images that permits initiation of the movement is obviously different from that which permits its completion, Mitchell (131, 132) suggests that different central mechanisms are involved in these two phases of eye movement. Mitchell (132) also considered the extent to which the limiting image dissimilarities within which vergence eye movement can be initiated are similar to those that permit the images to be subjectively localized in depth. The fact that the subjective reports of depth agreed closely with the frequency of vergence eye movement responses suggests that similar neural mechanisms for the detection of retinal disparity are used for the two functions. The initiation of vergence eye movements did not require simultaneous presentation of the two targets. Mitchell (132) found that stimuli were effective even though they were separated by up to 100 msec in time. This is about the same as the maximal temporal difference for stereoscopic depth perception. Ogle (137) found that stereoscopic depth experience deteriorated with increasing time delay between presentations to the two eyes, the rate of deterioration increasing with the magnitude of the angular disparity. With delays longer than about 100 msec, there was no stereopsis regardless of the disparity. An estimate from the data of Julesz & White (107), however, gave a value of about 11 msec for the temporal disparity limit using random-dot stereo targets (see above).

Recent evidence suggests that the mechanisms for coarse stereopsis and for the initiation of vergence eye movements in man both involve the visual cortex and require intact interhemispheric connections. Westheimer & Mitchell (186) studied the vergence responses to disparity stimuli in a subject whose corpus callosum and anterior hippocampal commissures had been surgically sectioned (63). The subject was suddenly presented with a target nearer than the fixation point such that an image fell on the temporal retina of each eye 2.2° away from the fovea. Eye movement recordings showed that the subject did not converge but merely made a lateral conjugate movement to fixate one of the targets. Convergence movements were obtained, however, when the target was presented to one side of the medial sagittal plane so that the disparity information from the two eyes was transmitted to the same cerebral hemisphere. Presumably the same hemisphere could not receive this information in the absence of interhemispheric connections so that correct responses to bilaterally presented disparate stimuli could not be made. The same subject was later tested for stereopsis by Mitchell & Blake-

more (133), who presented a vertical slit in the peripheral visual field, 5° to the right of the fixation point, with a disparity of 2° . He made correct estimates of "closer to" or "farther away than" the fixation point in 16 out of 20 exposures. With the slit stimulus in the middle of his visual field, again with 2° of disparity, the subject was unable to make responses significantly different from chance. The disparity values used in the above studies, however, would almost certainly be outside the range of fine stereopsis so that what has been studied is coarse stereopsis.

The evidence for an interhemispheric pathway for binocular integration in man is further strengthened by Blakemore's (18) study of another subject who had suffered accidental midline section of his optic chiasma. The subject could still make rough depth judgments when a slit was briefly exposed in front of his fixation point even though its images fell upon temporal retina in both eyes and therefore projected separately to the two hemispheres. The disparities used ranged from 0.5° to 7° . The nasal retinas were blind and he could make no such judgments for divergent disparities. Observations on the same subject also suggest that the interhemispheric pathway used for depth perception may also be involved in the control of vergence eye movements. Once again, these observations relate to coarse stereopsis and neither in this instance nor in the case of the subject without interhemispheric connections is the conclusion justified (18, 133) that fine stereopsis does not depend upon a nasotemporal overlap projected bilaterally along each optic tract (see below). It may well be that even fine stereopsis (and particularly global stereopsis) ultimately does depend upon intact callosal connections, but this is not to say that bilateral pathways projected from the two eyes via an optic tract to the one hemisphere do not provide an essential initial neural mechanism.

At first thought, the two kinds of stereopsis (fine and coarse) discussed above might be regarded as two grades of the same phenomenon. The differences between them, however, suggests that two different neural mechanisms are involved. Fine stereopsis seems to require a highly specific pattern-matching process involving very local features in the two images and operating over a very narrow range of spatial disparities. Fine stereopsis also seems to be closely linked with the phenomena of binocular single vision and retinal rivalry. By contrast, coarse stereopsis is a much less specific process which can operate on visual configurations, quite dissimilar in form, luminance, and contrast and separated by several degrees in spatial position. Coarse stereopsis is a phenomenon of double images and seems commonly to operate beyond the range of the suppressive action of binocular rivalry. Kaufman (108) found that binocular rivalry between contours fell to a low level at 2° disparity. Possible neural mechanisms for these two forms of stereopsis will be discussed below.

Mixed forms of stereopsis.—There is no reason to believe that the neural mechanisms underlying fine and coarse stereopsis cannot operate simultaneously. If this is the case, it would explain a number of seemingly con-

tradiictory phenomena such as those described by Kaufman (110). Among others, he described a new stereo pair consisting (in part) of two identical rectangular arrays of letters of the alphabet. The left array was bordered on the right side and bottom by single rows of letters, and the right array was bordered on the opposite sides by two rows quite different from those for the left array. When viewed stereoscopically the effect is rather like viewing Julesz's random-dot patterns. The central array is seen in depth through the outer border. The major difference between this stereogram and the more usual kind is that in this case there is no disparity between identical forms at any place in the binocular pattern. The end columns, however, have different forms, and since they fall on disparate places on the two retinas, both are either nasal or temporal when the inner region is fixated. Their cortical representations may therefore terminate in different cerebral hemispheres. Nevertheless, Kaufman concluded that the depth effect was due to disparity between the quite dissimilar uniocular end columns. The suggestion can now be made that the phenomenon described by Kaufman is an example of coarse stereopsis combined with normal binocular single vision for the central array as would occur when reading a page of print. Kaufman's observation is interesting in that the end columns containing the different letters can be separated by as much as 7° with the depth effect still present. This disparity limit is the same as that reported by Westheimer and his colleagues (186, 187) for the initiation of vergence eye movements to quite dissimilar targets.

Kaufman's depth effect, as described above, did not occur if the central arrays were composed of independent sets of letters, presumably because of rivalry effects. Many of the other phenomena described by Kaufman (109, 111) can be explained in terms of a fine stereoscopic mechanism, involving local matching and fusion of similar letter forms with local suppression of dissimilar forms, combined with a coarse stereoscopic mechanism which can tolerate the simultaneous perception of dissimilar letter forms or groups of letters separated by quite large disparities. Kaufman (110) also raised the following problem embodied in Julesz's concept of global stereopsis. Since the two stereo fields may contain a large number of form-similar picture elements or micropatterns, what cerebral mechanism provides the unique reference position so that the proper binocular matches are made over the whole target?

STEREOPSIS IN ANIMALS

The use of random-dot patterns enables stereopsis in animals to be studied by behavioral methods thereby providing a very important link between neurophysiology and psychophysics. Bough (21) has recently combined the random-dot technique with standard operant conditioning procedures to provide convincing behavioral evidence for stereoscopic vision in macaque monkeys. Random-dot patterns were cast onto a screen through oppositely polarized circular polarizers mounted on the projectors. Goggles

with polarizers worn by the monkeys confined the stimuli to separate eyes. The binocular appearance of the standard set of stereo targets was of an inner square lying behind the surround. One test required the monkey to discriminate between two stereo sets which differed only with respect to the relative depth of the inner square. The monkeys' performance was at least as good as that of the average naive human observer tested on the same apparatus. The neural mechanisms that have recently been proposed as a basis for stereopsis have relied almost entirely on evidence obtained from the cat. It is a serious criticism of this work that there is as yet no behavioral evidence for stereopsis in this animal. The application of the random-dot technique to the cat should resolve this criticism.

NEURAL MECHANISMS FOR STEREOPSIS

Microelectrode recording from single cells in the visual cortex has led to exciting new advances in our understanding of the neural mechanisms concerned in binocular single vision and stereopsis. The decisive initial step in the development of these ideas came with the observation that the majority of the cells in the striate cortex can be independently driven from both eyes so that a binocularly activated neuron has two receptive fields, one for each eye (87, 88). Relatively little interaction between the pathways from the two eyes takes place below the level of the striate cortex (cf. 153, 158). The two receptive fields of cortical cells have approximately corresponding positions in the contralateral hemifield and are of the same size and arrangement so that whatever stimulus is the most effective for the one eye—in form, orientation, and direction and speed of movement—is also most effective for the other eye. The cells differ, however, in their ocular dominance, i.e. in the degree to which they respond to one eye or the other. Subsequently these observations have been confirmed in all essentials (2, 142), and in particular, the degree to which the receptive fields do occupy exactly corresponding positions has been examined in some detail (2, 100, 134). The analysis of receptive field correspondence and studies of binocular interaction by stimulating the two receptive fields in varying degrees of alignment (11, 81, 143) have led to the development of a detailed neurophysiological theory for binocular single vision and depth discrimination (7-9, 100). Most of the work has been done with the cat as the experimental animal, but important new data have recently become available for the monkey (21, 95). The theory outlined below is primarily concerned with fine stereopsis.

A NEUROPHYSIOLOGICAL THEORY FOR BINOCULAR SINGLE VISION AND STEREOPSIS

An outline of the Joshua-Bishop theory (100) will form the basis for an account of the various developments that have occurred in this field. In this section the emphasis is on the theoretical aspects of these developments rather than on the experimental data. For details of the experimental data, the reader will be referred to the original papers, and only some of the key

observations will be mentioned here. Before we embark on this course, however, it will be helpful to define some of the terms that have been used in the development of the theory.

Definition of terms.—The receptive field of a visual neuron is the region of the visual field (tangent screen) over which firing of that cell can be influenced. The visual direction of a receptive field (receptive axis) is the line of sight through the region in the receptive field giving the maximal discharge to an optimal stimulus. This site in the receptive field approximates closely to its geometric center (11) (cf. 2, however). The two receptive fields of a binocularly activated neuron are in accurate register at that depth in space where the two receptive axes cross. The eyes are directed towards a fixation point when the maximal number of binocular receptive field pairs are in register at that point. There is also a plane for which the maximal number of binocular receptive field pairs are in accurate register. This plane, which by definition includes the fixation point, is analogous to the horopter in binocular psychophysics, and receptive field pairs are said to be corresponding when they are in accurate register in this plane. Receptive fields which are in register in planes either closer to the animal or farther away are said to show receptive field disparity. It is important to point out that, according to this usage, receptive fields which are in register are not necessarily corresponding. The terms "receptive field correspondence" and "receptive field disparity" have been defined in the above manner to facilitate development of the theory. To attempt a more precise definition of these terms, say with respect to the Vieth-Müller circle, introduces more problems than it solves and there still remains the difficulty of defining the fixation point for an experimental animal, particularly the cat (12, 181). In respect to these terms, it is also important to distinguish between retinal image disparity and receptive field disparity. The former concept is based solely on geometrical optics, whereas the latter is a neurophysiological concept primarily concerned with the detailed organization of the retino-cerebral pathway.

THE THEORY

One of the basic concepts is that of receptive field disparity. Only a proportion of the receptive fields of binocularly activated striate neurons occupy exactly corresponding positions in the visual field—the majority show receptive field disparity (2, 100, 134). Consider a set of receptive field pairs plotted on a tangent screen near the center of gaze (fixation point): if, in this small region, the fields for the one eye are moved to a common point, the receptive field centers for the other eye do not translate to a corresponding single point but are scattered about that point. Thus, every set of receptive field pairs having receptive fields for the right eye with a common visual direction have receptive fields for the left eye that show a two-dimensional, scatter about the exactly corresponding point. The above description applies equally well if the two eyes are interchanged. Because the experi-

mental observations relate to single cells or relatively small groups of cells, the theory is put forward in terms of small samples even though it is obvious that the phenomena of binocular vision must involve large populations of neurons. The fact that there are a very large number of cortical cells leads to the idea that there must also be a very large number of reciprocally overlapping, partially shifted distributions of receptive field disparities.

For receptive fields near the center of gaze the scatter of disparities is radially symmetric, having a standard deviation of 0.5° (100, 134). With increasing retinal eccentricity away from the fixation point, the distribution of the receptive field pairs undergoes two important changes: (a) the plane in which the maximal number of receptive field pairs are in accurate register curves round to become concave towards the animal rather like the horopter in man; and (b) for horizontal eccentricities there is a gradual increase in the standard deviation of the horizontal scatter while that of the vertical scatter may show a slight decrease. By contrast, both horizontal and vertical scatters remain unchanged for vertical eccentricities up to 20° from the visual axis. Barlow, Blakemore & Pettigrew (2) reported rather larger receptive field disparities (standard deviation of 1.5° for the horizontal and 0.51° for the vertical). The larger scatter is partly explained by the fact that their receptive fields were retinally eccentric up to 15° . Thus, we can expect a larger scatter as a result of the changes in the pattern of distribution of the receptive field pairs as described above and, in addition, plotting errors increase with the larger receptive fields in the periphery. These factors, however, are still not sufficient to account for the larger scatter of Barlow et al (2).

Binocular single vision.—Binocular depth estimates are based on horizontal disparities between the images in the two eyes. The estimates are made by striate cells from the information that converges upon them from the two receptive fields associated with each cell. Because of the small size of the receptive fields, especially in the center of gaze, these estimates are based on very localized features of the two retinal images. The discharge center of receptive fields of simple neurons in the cat striate cortex may be as small as $0.3^\circ \times 0.3^\circ$ (11) and even smaller receptive fields have been reported for the monkey (91). Most of our knowledge of binocular interaction on cortical cells refers to the class of simple cells (11, 143). The essential stimulus for these cells is a brightness change across a short contour segment having an optimal orientation and direction and speed of movement. For binocular stimulation the brightness levels may differ for the two eyes but the sign of the brightness change must be the same. Accurate feature-by-feature pairing takes place because the two receptive fields of the one cell have the same highly specific stimulus requirements for its discharge. Thus, each cortical cell selects for comparison those particular parts of the images in the two eyes that belong to the one feature of the external object. Once again it is important to emphasize that, because of the small size of the receptive

fields, the form similarity of the disparate images is on a very restricted local basis, possibly only a few minutes of arc across in the case of central vision in man.

With monocular stimulation, the striate cell's discharge is actively suppressed by even small departures from the optimal stimulus complex, particularly if the stimulus is not accurately located in the discharge center of the receptive field (80). Thus, even for monocular stimulation, it is likely that the normal visual environment will, at any one time, provide optimal stimuli for only a very small proportion of the striate cells, the remainder being actively shut off by stimuli inappropriate for excitation. Because the two receptive fields of a striate neuron have the same highly specific stimulus requirements, there is even less likelihood that, under binocular viewing conditions, the pair of fields will be simultaneously activated by two separate optimal stimuli. Under natural conditions, therefore, if the *one* optimal stimulus is to discharge the cell, it must be presented to the two receptive fields at the same time. This will occur only when the stimulus is applied at that depth in space where the two receptive fields are in accurate alignment, i.e. where the two receptive axes cross. Binocular single vision follows from the fact that both receptive fields are reporting the *same* stimulus feature at the *one locus*. Since only a single object feature is being detected, its binocular visual direction must be a compromise between the separate directions of the two receptive axes.

At depths somewhat closer to and farther away from this site, the two receptive fields are slightly out of register and the possibility arises of the cell being stimulated either by two separate stimulus features or by the same feature at slightly different times. It is suggested that double vision is prevented in these circumstances by the marked mutual inhibition that occurs between the two receptive fields (11). It is presumably this mutual inhibition that prevents double vision within Panum's fusional area. Outside this local area the possibility of double vision arises because the two receptive fields become sufficiently misaligned to act independently of one another. Even so, in the normal environment, mutually inhibitory influences will still occur because the two receptive fields will now be continually presented with dissimilar and inappropriate stimuli. This presumably accounts for the fact that we are usually unaware of double vision outside Panum's fusional area. By analyzing the distribution of the receptive field pairs at different retinal eccentricities it has been possible to arrive at constructions for the cat analogous to the horopter and Panum's fusional area in man (100).

We do not ordinarily see double in the vertical direction in the sense in which we can easily see double in the horizontal direction. Because of the horizontal disposition of the two eyes it follows that retinal image disparities in a vertical direction can ordinarily arise only because of a lack of correlation between the motions of the two eyes. However, these movements are always sufficiently well correlated so that the image disparities in the vertical direction that do arise probably fall within Panum's fusional area. In man it has

been found that the retinal image disparities produced by binocularly random eye motions have about the same magnitude as those associated with Panum's fusional area (54, 166). Furthermore, the fusional mechanisms operate in both vertical and horizontal directions and do not have the directional anisotropy that characterizes stereopsis.

The neural basis for single vision outlined above clearly requires that the mutual antagonism between the two receptive fields of a cortical cell take place with receptive field misalignments in both vertical and horizontal directions. The experimental evidence in support of the theory is unsatisfactory to the extent that it is based almost entirely on simple striate cells. The receptive fields of these cells mutually inhibit each other only when the misalignment is in a direction at right angles to the preferred stimulus orientation (11). Mutual inhibition in the two directions at right angles might be brought about by pooling the output of simple cells onto hypercomplex cells of the kind described by Hubel & Wiesel (89). In other respects, however, the occurrence of vertical receptive field disparities does provide an explanation for the fact that the fusional mechanisms for single vision operate equally on both horizontal and vertical image disparities, making the dimensions of Panum's fusional area in the vicinity of the fixation point about the same in the two directions. This would necessarily follow from the presence of equal distributions of horizontal and vertical receptive field disparities if each cortical cell reported a single stimulus feature at the one site by firing when and only when their two receptive fields were in accurate register. To understand the above argument it is essential to appreciate the difference between receptive field disparity and receptive field misalignment. The former term refers to the organization of the retino-cerebral connections whereas the latter has to do with the relative positions of the two eyes. Receptive fields which are corresponding and receptive fields which are disparate can both show misalignment. Just as with receptive field disparity, receptive field misalignment has, of course, to be distinguished from retinal image disparity.

The primary role of vertical receptive field disparities is probably to take account of the random eye movements of binocular fixation. If all binocular receptive field pairs were corresponding in a vertical direction, the binocular control of the eyes would have to be extraordinarily precise to keep the receptive fields in register. Because of vertical disparities there will be, within limits, other receptive field pairs to take over the reportage of a stimulus feature when random eye motions have displaced the receptive field pair that was formerly in register at that site. The suggestion is made that binocular fixation will oscillate about the position of maximal receptive field alignment in both horizontal and vertical directions. Like vertical receptive field disparities, the vertical extent of Panum's fusional area could have developed as a consequence of the binocularly random eye movements in the vertical direction.

Depth discrimination.—In general, cortical cells respond only if the

orientation of the bar or slit stimulus is optimal and, under binocular conditions, the response may fall off markedly if the receptive fields are misaligned in the direction at right angles to this orientation. As we have pointed out above, this is particularly the case with simple cells, which are the cells that have been studied most intensively so far (11, 80). Because the two eyes are in the same horizontal plane, only horizontal retinal image disparities provide depth information. Thus striate neurons can make binocular depth discriminations only if their two receptive fields are sensitive to misalignments in a horizontal direction, and this applies whether the receptive field disparity is in a horizontal or a vertical direction. Thus, cells with vertically disparate receptive fields can still be sensitive to horizontal misalignments of their fields. The receptive field disparity distributions provided by Nikara, Bishop & Pettigrew (134) and Joshua & Bishop (100) cannot be regarded as an entirely satisfactory basis for a theory of stereopsis on the grounds that the members of each receptive field pair have been plotted under monocular stimulating conditions. Thus, the distributions include cells that respond best to horizontally oriented bar or slit stimuli moved vertically across the receptive field. Some cells of this kind, particularly simple cells, are very insensitive to horizontal separation of the receptive fields, particularly if the stimulus is long compared to the size of the receptive field (80). Despite the technical difficulties involved, it is to be hoped that distributions will become available of receptive field disparities based on cells that are specifically sensitive to horizontal misalignments of their fields.

Neural mechanisms for global stereopsis.—There are clearly many parallels between the neural mechanisms outlined above and the phenomena of local stereopsis. While the neural theory may account for many aspects of local stereopsis, it is still quite inadequate as a basis for an understanding of global stereopsis. The problem of global stereopsis is likely to be particularly difficult if it requires for its solution an understanding of the neural mechanisms that make form discrimination possible. As yet we have only a very rudimentary knowledge of these mechanisms. Even the most complex cells so far studied in the higher visual areas of the cortex seem still to be concerned with the elements of form rather than organized wholes. The problem would be much more tractable, however, if it could be shown that global stereopsis depends upon the matching of clusters of picture elements or contour segments that all have the same disparity value and are in some kind of regular array. In other words, the nervous system may attempt to align the receptive fields so as to detect the maximum number of similar contour elements with the same disparity value and situated close to one another (cf. 106). Such a level of neural organization might be beyond that responsible for local stereopsis but short of that required for the discrimination of total forms.

Pettigrew, Nikara & Bishop (143) have described complex cells that

seem to have some of the properties that would be required for global stereopsis. The receptive fields of complex cells are usually fairly large, and the suggestion has been made that their properties are derived from the pooling of the outputs of a number of simple cells (88). The receptive fields of the simple cells would be spatially offset with respect to one another but otherwise would have the same optimal stimulus requirements. Based on binocular interaction studies, some complex cells behave as though their inputs were from a number of simple cells all having the same receptive field disparity value (143). Under binocular viewing conditions such cells would be discharged by a series of similarly oriented contours situated at the same depth in space.

Neural mechanisms for binocular depth discrimination in monkey.—Recently, Hubel & Wiesel (95) have found cells sensitive to binocular depth in area 18 of the macaque monkey cortex. This finding is of particular significance in view of the behavioral demonstration of stereoscopic depth perception in the monkey (21). No binocular depth cells were found in area 17, and even in area 18 the rather surprising observation was made that the proportion of binocular depth cells increased as the electrode moved out into cortex having an increasingly peripheral retinal representation away from the vertical midline. The most common type of binocular depth cell either failed to respond or responded only weakly to monocular stimulation, whereas appropriate stimulation of the two eyes together produced very brisk responses. Hubel & Wiesel also reported the phenomenon of receptive field disparity and noted that the displacement of the field in one eye, relative to the field in the other, was usually at right angles to the receptive field orientation. Vertically oriented fields were thus horizontally displaced, whereas with oblique fields there was a vertical component to the disparity as well. Binocular interaction of the receptive fields was maximal with misalignment of the fields at right angles to the receptive field orientation and minimal in the direction of the orientation. The cell gave a facilitated response over a range that was small compared to the dimensions of the receptive field. As yet, however, insufficient data are available from the monkey to warrant comparison with the data from the cat.

VERTICAL MIDLINE STEREOPSIS

The neural theory of stereopsis outlined above depends upon fairly precise retino-cerebral connections from approximately corresponding retinal areas that come together as a result of the partial decussation at the optic chiasma. This decussation requires the division of the retina into nasal and temporal parts. The classical description of the nasotemporal partition is that it takes place along a sharp vertical line passing through the center of the fovea. If this were the case it would be a grave handicap to the neural theory of stereopsis because it would mean that the receptive field disparity mechanism could not operate in respect to object points located between the

two visual axes (7, 17). The distribution of receptive field disparities would be sharply limited by the nasotemporal line so that a cortical neuron having a receptive field for the left eye located to one side of the line could not have the field for the right eye on the other side of the line. Hence, information about object points between the visual axes, immediately in front of or behind the fixation point, would be available to each cerebral hemisphere only from the one eye. A sharp division of the retina would provide particularly telling evidence against the neural theory since it is in the central region near the fixation point that stereopsis has its greatest value.

There is now clear-cut histological and neurophysiological evidence in the cat for a vertical strip of nasotemporal overlap by which the right visual cortex receives information via the right optic tract from both sides of the retinal midline of both eyes and vice versa for the left visual cortex. By sectioning one optic tract in the cat and observing the pattern of degeneration of retinal ganglion cells, Stone (172) showed that both optic tracts were supplied by ganglion cells in both eyes from a median strip of nasotemporal overlap 0.9° across. By plotting receptive fields for the two eyes from neurons in the lateral geniculate nucleus (LGN) and visual cortex, it has now been demonstrated that neurons at both these levels have receptive fields situated in the "wrong" hemifield just over the vertical midline. The earlier evidence for such an overlap at the geniculate (113) and cortical levels (100, 134) was subject to the criticism that there was doubt about the precise location of the vertical midline. This objection is avoided by recording from the thalamus (154) and the visual cortex (17, 119) on the two sides in the one animal. A nasotemporal strip of overlap projects to the region of the junction between the LGN and medial interlaminar nucleus at the thalamic level and the region of the border between areas 17 and 18 in the visual cortex. The magnitude of the overlap is approximately the same at both geniculate and cortical levels. For the cortex the distribution of the receptive fields on the "wrong" side of the midline (standard deviation 0.5°) was the same as might be expected on the basis of the phenomenon of receptive field disparity (17, 119; cf. 7).

The overlap at the cortex survives section of the corpus callosum (119). There is, nevertheless, convincing evidence that the cortical boundary zone across the 17/18 border receives a projection from the opposite hemisphere (5, 6, 27, 90). The transcallosal fibers, therefore, reinforce a nasotemporal overlap that is already present in the visual cortex as a result of the projection along the optic tract and radiation of the same side. The suggestion has been made above that there may be two kinds of stereopsis, fine and coarse, and the further suggestion can now be made that the two pathways we have just described might provide the basis for these two types.

From observations they made on two subjects, one with transection of the optic chiasma (18) and the other with transection of the corpus callosum (133), Blakemore & Mitchell concluded that the nasotemporal overlap that was projected via an optic tract was not an important factor for stereopsis in

man. Their argument, however, loses its force if a valid distinction can be made between fine and coarse stereopsis. If receptive field disparity, as we have described it, is the basis for stereopsis over the retina generally, it seems rather unlikely that an entirely different mechanism would apply for the region in the immediate vicinity of the vertical midline and all the more so when the necessary receptive field disparity mechanism is present in the overlap region. The quality of the stereopsis based solely on transcallosal links seems quite unlike the kind of stereopsis to be associated with the midline in the vicinity of the fixation point. It is possible that local stereopsis depends upon a local receptive field disparity mechanism and that both global stereopsis and coarse stereopsis require transcallosal links, though not necessarily the same links. In this case global stereopsis would require both the direct pathway for overlap and the transcallosal fibers to be intact.

CELL COLUMNS FOR BINOCULAR DEPTH DISCRIMINATION

From their observations on area 18 in the monkey, Hubel & Wiesel (95) have made the suggestion that cells representing a given stereoscopic depth relative to the surface of fixation are grouped together and segregated from cells that are not particularly concerned with depth. The idea that cells concerned with depth discrimination are segregated into columns extending from surface to white matter is an important one, but much further work is needed before it can be regarded as established.

CONCLUSION

As a result of the interaction of neurology and psychology, a radical change has been in progress over the past 10 to 15 years concerning our understanding of the nature of spatial vision. On the side of neurophysiology the contribution has been partly the result of technical developments, particularly single-cell recording, but also to a large extent it is a result of the growing realization that an understanding of the functions of the visual system requires a thorough appreciation of visual psychophysics. On the side of psychology there is still much uncertainty regarding the interpretation of many of the classic experiments in spatial vision such as the wearing of inverting spectacles. It is possible that neurophysiological studies will decide between the conflicting interpretations.

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PERCEPTION¹

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This review is orientated around sources of perceptual information. It will deal with information integrated across time, multiple sources of information, and organization. In addition, studies of the process of information selection will be considered in a final section on attention. The review is restricted mainly to work published between January 1968 and December 1969. In general, the review covers work on perception but not sensation. However, this line is not strictly drawn. The review emphasizes work done with adult human Ss and does not survey developmental, comparative, and physiological studies.

Obviously it has been necessary to be severely selective. The topical organization was the primary basis of selectivity. Numbers of excellent studies were omitted only because they didn't fit. Others were omitted simply for lack of space. A final unexpected and capricious selectivity was imposed by the occasionally erratic mail service between the authors in the USA and Uganda.

INFORMATION INTEGRATED ACROSS TIME

Perception of time.—Elapsed time itself is a source of information. Recent studies of temporal judgment tend to be atheoretical. Many are methodological investigations; others attempt to manipulate temporal judgments by stress or arousal; but few go beyond a descriptive analysis of time perception.

A typical methodological study is that of Hornstein & Rotter (137). They compared three methods of estimating short (2–29 sec) visual time intervals: 1) *verbal estimation*; 2) *production* of a duration specified by experimenter; and 3) *reproduction* by S of duration produced by experimenter. Verbal estimates were found to be too long, while production and reproduction judgments tended to be too short. The authors suggest that the amount of activity in the method may be important. However, Goldstone (117) found that when time estimation involved intersensory comparisons of light and sounds,

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method of estimation made no difference. Sound intervals were judged longer than equal light intervals by all methods.

Successive reproductions of short time intervals have been found to grow progressively longer (44, 187, 272). Von Sturmer (272) was able to manipulate this effect by varying payoff conditions, using a statistical decision model. The method of production does not yield a similar consistent trend. For example, Emley, Schuster & Lucchesi (74) observed both overestimation and underestimation, depending on the length of interval and number of trials at each trial. Similarly, Ross (243) reports time estimation to be differentially affected by different patterns of light stimulation.

Investigations of the effects of various independent variables on time perception are typified by the numerous studies dealing with effects of arousal or stress (31, 122, 171, 245). Cahoon (31), for example, hypothesizes that a state of high arousal is accompanied by a faster subjective time rate, i.e. overestimation of time intervals, than a state of low arousal. His hypothesis is supported by data from conditions of natural arousal (anxiety), but not by data from induced arousal (shock). Legg (171) investigated the hypothesis that the alpha cycle forms the unit of subjective time, but found no relation between alpha frequency or duration and length of time judgments.

Motion perception.—This represents the most complete integration of information over time. Some investigators have attempted to define a general theory of motion perception. Kinchla & Allan (155), for example, propose that movement perception be treated as a problem in position memory. An observer compares the position of an object at one point in time with his "memory" of its position at an earlier point in time. Such comparisons of position are limited most by the eye-positioning system. A mathematical model is proposed for conditions of absolute movement (movement of an object in a homogeneous field), which, it is suggested, can be elaborated to include relative movement perception (movement of objects in a heterogeneous field) as well.

Gibson (104), on the other hand, rejects any assumption that the displacement of the retinal image over the retina is the effective stimulus for an impression of motion. He asserts that the effective proximal stimulus for motion perception is not the motion of the retinal image, but the change of pattern or motion in the retinal image. A theory of optical information for perceiving motion should deal with continuous transformations of form and texture, and disruptions of texture. For example, Kaplan (152) has found that accretion or deletion of texture on one side of a margin is a sufficient stimulus for the preception of occlusion of one surface by another nearer surface.

Several other studies emphasize a relation between perception of motion and transformations of the optical array. Johansson (147) found that sinusoidal changes, in a continuous brightness gradient across a field, were sufficient to elicit visual tracking responses. Mates (190) found that the velocity threshold for a moving rectangle decreased as a function of number of refer-

ence lines. Rock (235) found that *Ss* passively moved in the dark, but able to see single luminous circles, attributed the relative movement to the environment. When an entire array of vertical lines was passed, *Ss* attributed the movement to themselves.

Other investigators of motion perception study separate specifically defined types of motion, such as real movement, apparent motion, and auto-kinesis. Graham (119), for example, investigated the factors involved in the perception of constant rate movement of single stimuli. Among the variables he studied was the effect of the number of reference lines, the length of the stimulus line, and the stimulus intensity on the velocity threshold. Subjects were asked to fixate the front edge of a vertically descending moving stimulus and to respond to the presence, or absence, of movement at any time during a 6-sec trial. The velocity threshold decreased with increase in number of objects in the field, and with increase in stimulus intensity, but it increased with increase in stimulus length. Graham suggests that velocity threshold changes involve an inhibition mechanism in the retina. Movement is determined by conditions of positive and negative contrast at the leading and trailing edges of the object.

Graham also reviewed the classical explanation of the rotating trapezoid (window) illusion, according to which, linear perspective information determines the relative depth of the edges of the trapezoid, since motion parallax information is ambiguous. Specifically, the large edge of the trapezoid is always seen as closer than the small edge because its projected visual angle is always larger, and hence it appears to oscillate rather than rotate. Jansson & Borjesson (144) reanalyzed the proximal stimulus of the trapezoid illusion and show that, in fact, there is sufficient information for unambiguously determining direction of rotation. A number of investigators have modified the traditional trapezoid illusion to examine the interaction of other depth information with the ambiguous motion parallax (24, 32, 49, 97). Canestrari & Farne (32) reasoned that any cue which would make one part of an object appear farther away than another should elicit an illusory perception of oscillation rather than veridical rotation. They demonstrated this for relative brightness, interposition, and texture differences.

Apparent motion is one form of perception of motion in the absence of object displacement. The parameters of visual apparent movement have been well studied, and the conditions for occurrence of tactual apparent movement are now receiving attention (17, 251). Sherrick (251) stimulated contralateral sites on the arm with vibratory stimuli, using durations from 0 to 200 ms. As the stimulus duration increased, the interstimulus delay necessary for optimal motion first decreased then increased. Inhibition-suppression mechanisms have been used to explain apparent motion, and Kahneman (150) has attempted to explain metacontrast, usually considered an inhibitory phenomenon, as a special case of apparent motion. Weisstein & Growney (299) show, however, that visual angle, luminance, and duration affect these two phenomena differently. For example, increasing the visual angle between

stimuli has no effect on ratings of apparent motion, but diminishes ratings of metacontrast; at low luminances, increased duration of stimuli has no effect on apparent motion, but diminishes metacontrast.

Visual autokinesis, the illusory motion of a fixated point of light, may be related to felt changes in eye position (26, 56, 149, 217). Jordan (149) reports that during autokinesis the position of the eye undergoes a change in felt position. When Ss were asked to look straight ahead, during or after autokinetic movement, their eyes were moved to a direction opposite the apparent movement of the target. DeSisto & McLaughlin (56) similarly demonstrated a change in judgment of direction of gaze by asking Ss to look back to the starting point of the autokinetic target. All Ss made a large eye movement in the direction opposite to that of the horizontal autokinetic movement. They suggest that in autokinetic perception S is conscious of making a pursuit movement when no movement is occurring.

Perceptual aftereffects.—Recent research on modification of perception by artificial rearrangement has implicated eye movements and eye position registration (130, 212, 218, 257, 293). Ordinarily, image movement across the retina which is correlated with voluntary eye movements is not perceived as object movement. If this correlation is artificially changed, however, Ss will perceive object movement when they move their eyes. Hay (130) demonstrated that Ss can adapt to such an artificial correlation, perceiving less object movement after very short exposure. He arranged a target to move left and right in synchrony with S's nodding his head up and down. After one minute of tracking the pip, less object movement was perceived. The results suggested that a necessary and sufficient condition for adaptation was exposure to a new correlation between head and eye movement.

Among the more interesting theoretical developments in the area of adaptation to distortion is the work of Ebenholtz on adaptation to optical tilt (64-67). He has elaborated a comparator model in which the effective stimulus for adaptation is the difference between the immediately *present* optical stimulation and *previous* optical stimulation. In the case of artificial *change* of tilt, for example, the present optical flow pattern would differ from previous patterns. One deduction from this model is that if Ss are exposed to a distortion which is exactly the level of their adaptation reached after exposure to a previous distortion, they should show a decrease of adaptation. The stimulus for adaptation in this case would be the decrease of distortion from the initial level to their adaptation level. This was confirmed by Ebenholtz (64), as was an additional, intriguing deduction that equal changes in either direction from an initial distortion would result in equivalent changes in adaptation level [cf. Collins & Singer (42) and Freedman (96) for other theoretically relevant work].

Other studies of adaptation to distortion have been more strictly empirical in nature and have manipulated conditions for adaptation (29, 68, 91, 92, 197, 295, 300) or have studied transfer of adaptation in a variety of con-

texts (93, 165, 168, 226). Efstathiou (68), for example, investigated the time course of adaptation for Ss performing a hand-eye coordination task while wearing wedge prisms. Adaptation was a negatively accelerated function of exposure time, reaching asymptote after 15 to 20 min. Amount of adaptation was found to be a curvilinear function of amount of distortion. Adaptation increased as a function of lateral displacement up to about 13° , then decreased at a distortion of 15° . Representative of the transfer studies is Foley & Miyanshi's (93) investigation of interocular transfer. In their investigation, Ss walked up and down a corridor for 15 min with a base-up prism over their right eye. Their left eye either was occluded, viewed through plain glass, or viewed through a base-down prism. Tests for adaptation to displacement were conducted independently for the two eyes. When the left eye had been occluded, it showed adaptation equal to that of the exposed right eye. When the eyes viewed through opposite prisms, the left eye exhibited a relatively large adaptive change, and the right eye showed a smaller shift in the same (maladaptive) direction. This last result shows a limited independence of the two eyes in adapting to prismatic displacement.

In spite of years of research and hundreds of investigations, the classical Köhler-Wallach aftereffect (FAE) still appears tenuous. Its sensitivity to slight procedural changes makes it an unlikely clinical tool (301). In a controversy which concerned the relation between susceptibility to FAE and performance in a field dependency test, it was pointed out that FAEs are sensitive to differences in group vs individual testing, sex of *S*, maintenance of fixation and set (58, 140, 141, 221, 222). Dodwell & Gendreau (59) and Wertheimer & Skeets (302) also provide evidence for the effect of set on FAE.

McCullough's (194) technique of selective adaptation has become a very popular tool for investigating specificity in complex perceptual processing. In her original study, Ss were exposed to a prolonged period of alternating horizontal edges of one color and vertical edges of a second color. After such exposure, chromatically neutral horizontal and vertical edges appeared to have distinctly different tints tending to be complementary to the inducing colors. McCullough suggested the possibility of orientation and color specific edge detectors in interpreting her results. These investigations have recently been extended (128, 270, 274). Stromeyer (270), for example, found that aftereffects for both horizontal and vertical lines could be induced with prolonged viewing of vertical lines alone. He further found that the aftereffect tints were only red or green, depending on the inducing color. Using a very similar paradigm, Hepler (133) demonstrated a motion aftereffect which was color and direction specific. After exposure to an alternating sequence of upward moving red lines and downward moving green lines, Ss reported that neutral lines moving up or down had an aftereffect tint complementary to the tint they had during exposure.

All the above studies employing selective adaptation have used color "tagging" as a means of demonstrating specific perceptual mechanisms.

Other investigators have used increase of detection thresholds for test stimuli as a function of exposure to specific kinds of adapting stimuli to demonstrate specificity of perceptual mechanisms. For example, Sekuler, Rubin & Cushman (249), using a selective adaptation technique, found that the mechanism for detection of vertically moving horizontal striations is quite widely "tuned." Thresholds for detection of such movement are raised by prior exposure to lines moving over a wide range of direction. On the other hand, the threshold for detection of a stationary grating is quite narrowly tuned to that particular orientation. Other similar investigations of specificity of perceptual mechanisms include: size (19, 210), visual orientation (4, 105, 106), vibrotactile stimulation (127), olfactory stimulation (75), loudness (202, 296), and pitch (255). Cf. also Weisstein (298) for an interesting discussion of the relation between selective adaptation and visual masking.

Reversal of depth perspective with prolonged inspection occurs for a number of ambiguous figures. The one most studied is the Necker cube. Frequency of reversal has been found sensitive to such variables as visual angle (61), completeness of figure (259), instructional set (215), and amount of preliminary experience (224, 259). Cormack (45, 46) found that retinal disparity which reinforces one of the alternative perceptual organizations of the Necker cube reduces the frequency of reversal.

The duration of the spiral aftereffect has also been found sensitive to size of visual angle of spiral, to speed of rotation of spiral (43), to relation on retina between inducing and test stimuli (189), and to criterion of judgment (193). Masland (189) reports that the spiral aftereffect can be elicited by the appropriate test stimulus 20 hr after a 15-min exposure.

Temporal limits of perceptual information.—How long does perceptual processing take before it is considered integration over time?² Many investigations have been concerned with parameters affecting perception of simultaneity or temporal order (47, 102, 134, 206, 276, 294). Thor & Spitz (276), for example, investigated the temporal parameters for the perception of successiveness. They presented two stimuli successively at the same retinal locus and Ss judged which appeared last. Results indicated that accuracy was dependent on total processing time and not on the interstimulus interval alone. Discrepancies between objective simultaneity of light flashes and judgments of subjective simultaneity caused Corwin & Boynton (47) to ask whether subjective simultaneity is a transitive phenomenon. This is implied by their suggestion of a "simultaneity center" in the brain, where excitations from different visual paths must coincide. They confirmed this implication by measuring the interstimulus intervals required for perception of simultaneity between all pairs of three retinal points.

Closely related is the problem of rate of information processing. Eriksen & Spencer (79) suggest that the rate of processing is determined by the num-

² Current reviews of considerable relevant work on successive visual masking exist (151, 297) and this topic will not be reviewed here.

ber of discrete percepts processed per unit of time and by the amount of information processed from a single percept. Although the visual system can set limits on the number of percepts per unit time, the amount of information is not necessarily so limited. Many studies of amount of information processed have confronted methodological complications, including a time-energy trade-off, foveal location of elements, discriminability of multiple elements, and memory problems. In an attempt to circumvent some of these problems, Eriksen & Spencer used a visual search task in which Ss searched for the letter A in a sequence of letters. In a series of experiments, presentation rate between 5 ms and 3 sec had no significant effect on correct responding. The results seemed to preclude any serial encoding process, requiring instead a multichannel encoder or a filter model.

Eriksen & Spencer also suggest that limitations on rate of processing may be attributable to subprocesses involved in the informational flow. Eriksen & Collins (77) investigated the time required to process or apprehend the meaning of different report indicators. They concluded that many failures in selective attention are due, in part, to decay in the icon during the time required to process the associated indicator for responding. Spencer (262) determined backward masking functions with both a patterned and a blank mask for single and 12-letter circular displays. The results indicated that the patterned mask prevented completion of the hierarchical stages of processing, i.e. it controls read out time from an iconic store.

Several studies have indicated that rate of processing may vary as a function of the stimulus input (1, 39, 57, 204). Dick (57) assumed that a decrease in report accuracy reflects a slower processing rate and found that such decreases in accuracy occurred with increasing delay post-exposure for spatial report and for color (physical information), but not for class report of letters and numbers (identity information). Aaronson (1) examined the effect of a recall task on monitoring a series of digits for a critical item. Reaction time, an index of perception time, increased under the recall requirement, and serial perceptual processing also seemed to be occurring.

MULTIPLE SOURCES OF SENSORY INFORMATION

When information is received simultaneously from a number of sources, one source can degrade, accentuate, or bias the other source. Such modification will be termed interaction. In addition, studies of intermodal transfer and modality comparisons will be considered in this section.

Sensory interaction.—The first type of sensory interaction to be considered will be degradation of one type of stimulus information by another. The most common example is masking.² As in the past, there is currently a heavy concentration of research on auditory masking (22, 33, 69, 146, 305, 310). Jesteadt & Bilger (146) found that sensitivity to frequency change decreases with increasing noise intensity, even though the test tone sensation level, relative to the masked threshold, remains the same. In general, masking

effects diminish as the phase relations between the masking and masked sounds become more different. Carhart, Tillman & Johnson (33) showed that the masking effect of combined speech and white noise was greatest if both were presented monaurally in the same ear as the signal and least if both the speech and the noise were 180° out of phase to the two ears while the signal was in phase. These and various other combinations of two masking stimuli produced more masking than either speech alone or noise alone presented monaurally to the same ear as the signal.

Masking studies have also been conducted in several other sense modalities, e.g. vision (20, 281), touch vibration (108, 109), and taste (142, 269). Uttal (281) studied the effect of changing visual noise on letter recognition. Letter characters were painted in dots on a cathode ray tube and were overlapped with noise dots painted in rapid succession (intervals from 1 to 13 ms) at randomly varied spatial positions. As the interdot interval increased, recognition improved from 10% to 95% in a negatively accelerated function. Asymptote was reached at approximately 10 ms. Another interesting example is Gilson's (108) investigation of masking effects of vibratory tactile stimuli. When a vibratory test stimulus of 160 Hz was placed on the thigh, masking effects decreased as a function of longitudinal distance of the masking stimulus from the test site. It made no difference whether the masking stimulus was ipsilateral or contralateral to the test stimulus. The effect of longitudinal displacement could be partially offset by appropriate stimulation lead or lag of the masking stimulus, relative to the test—indicating the masking effect was basically a temporal rather than spatial phenomenon. When the index finger was used as a test site, then ipsilateral stimulation produced more masking than contralateral. In a second study Gilson (109) found that multiple maskers produced approximately the sum of the masking effect of the individual masking stimuli.

A second kind of sensory interaction to be considered is accentuation of one type of information by another. The traditional example is contrast (113, 182). Gogel & Mershon (113) employed the Gelb effect to investigate the influence of spatial separation of stimuli on visual whiteness contrast. This effect refers to the change in apparent whiteness of a black stimulus which is perceived as white under strong illumination in an otherwise dark field. When a small white stimulus is superimposed under the same illumination on the black, the apparent whiteness of that black stimulus is sharply reduced. The unique feature of the present study is that the large black stimulus and small white stimulus were separated in depth while remaining concentric in azimuth. The change in perceived whiteness of the black stimulus was less as the depth separation between the two increased. Since the configuration of the retinal image of the stimuli remains the same, the implication is that these contrast effects are mediated by a central mechanism.

Lateral interaction in the peripheral visual system has served as a mechanism for explanation of many visual contrast effects, including Mach bands (16, 25, 34, 143, 154, 260). Smith & Richards (260) determined a functional

velocity for spread of lateral inhibition in the human visual system. They examined luminance threshold changes for a single line after the onset of two flanking lines. When spatial separation between test line and flanking lines increased, an increasing delay was necessary for maximum threshold changes. The separations and delays suggest a propagation velocity of 0.65 deg/sec.

Two new kinds of Mach bands have been reported. Jacobson & MacKinnon (143) have described colored Mach bands produced with saturation step functions of complementary hues of equal brightness. Carterette, Friedman & Lovell (34) investigated an auditory counterpart of Mach bands. A previous result of von Bekešy had suggested that lateral interaction takes place on the basilar membrane, where particular sections are stimulated by narrow bands of noise. In the present study, 100 and 200 Hz bandwidths of noise were generated with very sharp attenuation beyond the passband frequencies. The masking capability of this noise was measured all along the frequency range, i.e. inside and outside the noise band. Sharpening discontinuities occurred at the cutoff frequencies analogous to both light and dark visual Mach bands.

Biasing of one type of information by another is the third type of sensory interaction considered here. Such biasing may occur when two normally corresponding sources of information about the world are incongruent. Such effects have been reported for conflicting information from eye convergence and azimuth on median plane judgments (196), from proprioception and vision on judgments of inclination (254), from vision, audition, and proprioception on judgments of azimuth (186, 219), from proprioception and gravity on judgments of orientation (38, 55, 131, 156, 286–290). Wade (286–290), for example, has shown that tilting the head or tilting the whole body while keeping the head and trunk in alignment produces an error in the opposite direction in judgments of visual vertical. The error with trunk tilted one way and head tilted opposite (so as to remain physically upright) was equal to the sum of the errors of body tilt by itself in one direction and head tilt by itself in the opposite direction. From these results and from additional data on aftereffects of prolonged head, body, and trunk tilt (54, 286, 291), and from data of labyrinth-defective Ss on perception of vertical (198), Day & Wade (54) argue that the constant errors in judgments of visual vertical are attributable to proprioceptive information from the neck and trunk as well as from the traditionally recognized otolith system.

Intermodal relations.—These have been investigated by crossmodal matching and by comparison of functioning in two modalities. Crossmodal matching studies can involve matching of information about the same stimulus from different modalities (89, 117), or matching of completely different stimulus variables (136, 188, 268, 271, 304). Fisher (89) asked Ss to match with proprioception the position of visual and of auditory targets and to match the position of a sound with that of a light etc. In a group of Ss tested on all pairs of the three modalities, there were significant constant errors in 73% of the cases.

Matching of different stimulus variables is exemplified by a study in which Ss matched the taste of beer to the pitch of a tone (136). Carlsberg Elephant Beer was matched to a higher pitch than Carlsberg Lager. In a more systematic vein, Mashhour & Hosman (188), assuming Stevens' psychophysical power function law, compared crossmodal matching functions with predictions made from magnitude estimation functions of the individual stimulus dimensions. For example, magnitude estimations were made for length and for finger span. From the exponents obtained, it was predicted that the exponent of the crossmodal matching function should be .87. The obtained exponent was .55. On the basis of 17 such comparisons, the authors questioned the validity of Stevens' law. However, Stevens (268) reanalyzed their data, correcting for regression effects and other artifacts, and found a reasonable fit between obtained and predicted exponents.

Most research comparing functioning in two modalities has employed touch and vision (35, 169, 209), although Baker & Payne (11) report a comparison of visual and auditory sequential pattern perception in which visual perception was superior. Lederman & Taylor (169) compared visual and tactual errors in perception of spatial position and orientation. In one of their experiments on spatial position, Ss had to reproduce the locus of a dot on a card which they had explored visually or tactually. Tactual errors were consistently larger than visual, but the pattern of the errors was very similar. Errors in both modalities were smaller for dots in the center or near the edges. The direction of errors was toward the corners. The pattern of errors for perception of orientation was more complex, but could be understood in terms of anchor effects.

MULTIPLE SOURCES OF INFORMATION—SENSORY INTEGRATION

Simultaneous bilateral stimulation.—Correlated inputs to two receptor surfaces of the same modality often fuse into a single percept, which includes information not experienced in the separate stimulation of either surface. The classical examples are stereoscopic depth and binaural directional perception. However, similar fusion and integration of information has been reported for tactual and other sense modalities. Thus vibrotactile stimuli, identical in frequency, phase, and amplitude, can be localized in space intermediate between the two areas of stimulation. Gescheider (101) found that a phase difference of 47° between two such stimuli applied to the ring and index fingers of the same hand produced a just noticeable shift in localization. Larger phase differences were required for threshold localization shift when stimulators were applied to opposite hands.

Some studies have been concerned with lateral localization in binaural hearing—particularly with the role of binaural temporal phase and intensity differences (63, 72, 99, 100, 126, 139, 244). Several of these studies focused on the trade-off relation between temporal and intensity differences. Hafter & Jeffress (126) explored the double auditory images which sometimes occur when both temporal and intensity differences (out of phase) exist between

the stimuli to the two ears. The "time difference" image is attributed to threshold-like sensitivity to the amplitude-time slope of the wave form, while the "intensity difference" image is attributed to the latency of the synchronous whole nerve action potential. It is suggested that the "time difference" image is mediated at the apical end of the cochlea and the "intensity difference" image at the basal end.

Recently interest has revived in the capacity to make monaural localization judgments and binaural localization judgments in the vertical plane (12, 30, 241, 242). Roffler & Butler (241, 242) found that vertical localization of auditory signals was possible with sounds which had frequency components above 7000 Hz. It appeared to depend on the use of the pinnae. A number of studies of monaural directional hearing also implicate the pinnae. However, while both localization in the vertical plane and monaural directional hearing are possible, precision is considerably less than binaural lateral localization (30).

Stereoscopic depth perception has been shown to be sensitive to a number of environmental variables. Temperature, wind, and water were demonstrated to reduce stereoacuity (158, 179), as does restriction of field of view (178). Target movement during judgment elicits idiosyncratic constant errors in stereoscopic judgments (180). Other studies have been more concerned with the process and mechanism of stereopsis (125, 153, 167, 227). For example, Rawlings & Shipley (227) investigated stereoscopic acuity as a function of horizontal angular offset of a fixed target size because traditional studies showing decrease in stereoacuity with increasing peripheral target angle confound object size with peripheral distance. In the present study, two peripheral targets were presented monocularly to each eye. The distance from fixation point to center of the two targets (offset) could be varied independently of the disparity in location of the two targets. The threshold of stereoacuity was greatest at 0° offset and increased as the targets moved peripherally. There was an apparent discontinuity in the curve of stereoacuity at about 6° offset and beyond that stereoacuity rapidly decreased.

Interesting effects occur when stereoscopic information conflicts with other depth information (107, 116, 247). Gogel & Newton (116) investigated distortions of depth perception elicited by differences in relative size, incongruent with binocular depth information. Playing cards, different in size but otherwise identical, were presented one above the other against a uniform background. The size differences elicited distance judgments which were a compromise between the true depth difference and that specified by the relative size difference. The depth distortion was also manifest to a reduced degree in judgments of the relative depth of two equal discs located in a vertical line between the playing cards. These results support a suggestion that conflicting depth information may distort, or structure, an entire spatial frame of reference.

When the same information is presented bilaterally to a sense modality, it could be processed as if the two sides were independent, dependent, or

partially dependent. Townsend (278) developed a probability model which made differential predictions for these possibilities. In a task to detect the presence of a target letter in a letter matrix, there is no information summation between the two eyes. Eriksen & Greenston (78) describe a somewhat similar experiment involving letter identification. At some interstimulus delays, presentation to corresponding retinal areas yielded a summative interaction.

When different information is presented simultaneously to the two eyes, rivalry or alternation may occur, rather than integration (23, 94, 164). Fox & Check (94) investigated whether the suppression in rivalry was general for all stimulation impinging on the momentarily nondominant visual field, or whether the suppression was selective. General suppression would, presumably, be reflected in slowness of detection of a moving stimulus presented to the nondominant field, since motion is an especially prepotent stimulus. Evidence of selectivity would be equally rapid detection of motion during suppression and dominance. Using a reaction time measure, motion detection was retarded in the suppression phase.

Integration of information in size perception.—There have been many studies of size perception itself (14, 21, 238, 240, 264, 282), but integration becomes relevant when the process of size comparison is investigated. Borozdina & Gippenreiter (21) recorded eye movements when Ss compared the size of two lines. The number of fixations was a generally decreasing function, as the length difference increased from below to above threshold. There was a slight increase in number of fixations at threshold.

Information integration is clearly manifest in the relation of size and distance perception. Epstein & Landauer (76, 166) investigated whether visual angle itself is a direct determinant of size perception. They showed that under reduced viewing conditions of meaningless disc stimuli, perceived size varied directly with size of visual angle and perceived distance varied inversely with size of visual angle, a result also obtained by Kunnapas (163). Such results are inconsistent with the size-distance invariance hypothesis and are not always obtained (110). That hypothesis would predict that either perceived size or perceived distance would vary with visual angle. The implication is that visual angle independently specifies perceived size and perceived distance and the two aspects of perception are not directly related.

The presence of familiar size information complicates the size-distance relation (111, 112, 115). Gogel obtained size and distance judgments of familiar objects when all distance cues except familiar size were eliminated. The size judgments showed a moderate to high correlation, both with the visual angle subsumed by the stimuli and with the real size of the objects. However, distance judgments were most closely correlated with the distance the real object would have to be to subsume the given visual angle. On the basis of these results, a two-stage process of perception is inferred. In the absence of distance cues, an assumed specific distance integrated with retinal and familiar size information yields a perception of size. Distance judgments are

then made inferentially, taking into account the degree of the off-size perception. The role of visual angle in the presence of the other distance information has been studied by Franklin & Erikson (95) and McDermott (195).

The classical and still intriguing example of lack of size constancy is the moon illusion. The systematic efforts of Rock & Kaufman (237) attributed the moon illusion to the differential registration of distance to the moon when it was on the horizon and at the zenith. Solkhah & Orbach (261) also reason that the moon illusion depends on registration of distance, but in their case distance is inferred from size constancy for familiar objects. However, some investigators continue to find evidence that there is a vestibular component, i.e. that the size discrepancy is related to changes in head-eye position relative to the body (301, 309). Wood, Zinkus & Mountjoy (307) simulated the moon illusion in a darkened room and asked Ss to match, in size, a standard disc over their head from a series presented at eye level. Their matches displayed a constant error in the direction predicted by the moon illusion, although considerably less than that usually obtained.

Rock, Hill & Fineman (236) reasoned that the perception of speed at different distances may depend on size constancy. If a far and near object are moving across the line of sight at the same speed, we perceive them veridically, in spite of the fact that their retinal images differ in speed. If the distance traversed by the two objects is perceived as equal, i.e. if there is size constancy, then there is a basis for speed constancy—the two objects traverse the same distance in the same time. Positive experimental results were obtained under conditions which ruled out a previous explanation of this constancy in terms of a frame of reference effect. Thouless (277) confirmed a converse hypothesis that a change of perceived speed, upon looking through binoculars, depended on lack of size constancy.

Over (208) has thoughtfully reviewed theories of geometric illusions, classifying them as physiological, information sampling, and experiential. In the latter category is Gregory's (123) explanation of a number of illusions which are related to size constancy. If one of two objectively equal line segments is seen as farther away, it should also be seen as larger because of "inappropriate constancy scaling." This idea has provoked a good deal of research on such illusions as the Müller-Lyer and Ponzo (37, 82, 85, 86, 138, 145, 173, 199, 263), most of which have been critical of the Gregory hypothesis. The most systematic examination of the Gregory thesis is that of Fisher (81, 90). Experiments with variant forms of the Müller-Lyer, Ponzo, and Orbison illusions fail to yield size differences predicted from the apparent differences in depth of the stimulus configurations, or fail to find relative size changes when there are reversals of perspective in the stimulus configurations.

The Ponzo illusion has received considerable additional attention (87, 90, 172, 213). Fisher (87, 90) describes experiments demonstrating a gradient of illusion from small to large as the comparison lines appeared closer to the apex of the angular context lines. Also, orienting the context lines in a left-

right direction produced less illusion than orienting them vertically. Finally, a single oblique line produced an illusion effect approximately half the magnitude of the two lines together.

Investigating the horizontal-vertical illusion of size (H-V), Avery & Day (10) found its magnitude to be dependent on the orientation of the two lines with respect to the retina and not the environment. Pearce & Matin (214) also find evidence that this illusion is related to position in the eye. The illusion decreases and reverses in direction as it is presented above and below the fovea. They suggest that dioptric properties of the eye may account for this variation, if not for the whole illusion. A haptic counterpart to the H-V illusion has also been explored (36, 273). Cheng (36) asked Ss to compare length of movements which were systematically varied with respect to their tangential-radial direction, their position with respect to the body (front to side), their distance from the body, and the angular separation between them. He found a pervasive radial-tangential difference; radial movements are overestimated with respect to tangential. Also more distant tangential movements are overestimated relative to nearer.

Some studies of illusions have been inspired by Ganz's (98) theory of contour displacement, which subsumes illusions and FAE under the same mechanism. Contours in close proximity, whether seen simultaneously or successively, are considered mutually inhibitory, resulting in phenomenal displacement away from each other. Robinson (234) and Wagner (292) have both argued that this hypothesis fails as a general explanation of illusions by offering counter-examples, while Dreistadt (60) and Ellis (73) present evidence supporting the relation between illusions and FAE.

Perception of distance or depth.—This in itself often involves integration of information from a number of sources. Distance perception has been studied directly with magnitude estimation procedures (40, 163, 275, 283). Cochran, Throop & Simpson (40) investigated the perception of distance of sounds in natural settings. They presented speech sounds to blindfolded Ss at distances between 1 and 29 m. The Ss estimated the distance relative to a standard sound. Their results for outdoor settings could be fitted to power functions with exponents between .69 and 1.10 (estimated from their graphs). Kunnapas (163) employed the traditional procedure of examining the effect on distance judgments of isolating visual depth cues. The relation between perceived and actual distance could be fitted by a power function for all conditions, except the most impoverished. The exponent of the power function increased from .82, with only visual angle and accommodation available, to .97 in a normal viewing condition.

Other researchers have concentrated on specific distance cues: familiar size (114, 207); height in visual field (62); inclination to line of sight (258). Ono (207) placed equal-sized drawings of an object with large familiar size and an object with small familiar size on the ambiguous surfaces of a 2-dimensional reversible figure. He found that the surface with smaller familiar

size was seen as closer to S more often than it would otherwise be. The effect was enhanced by increasing the size connotation of the drawings.

Context effects.—These effects exemplified by studies of adaptation level and anchor or reference stimuli also imply information integration (28, 132, 162, 185, 211, 229, 230). Helson & Kozaki (132) illustrated the effect of temporal context by showing that size judgments of stimuli were affected by the duration of exposure and size of anchor and other stimuli in the series. The direction of this effect was predictable from adaptation level theory. For example, if the larger stimuli were exposed for longer durations, the adaptation level was shifted towards the large end of the series and all stimuli were judged relatively smaller. Restle & Merryman (229) applied adaptation level theory to the prediction of a simultaneous size illusion. The apparent length of a line connecting two squares is diminished as the size of the squares increases. Adaptation level theory predicts that the judged size of the line will be equal to the physical size of the line relative to the weighted geometric mean of all the elements in the field. The form of results of category ratings of lines of various lengths with various square sizes satisfactorily conformed to the predictions.

ORGANIZATION AND CODING OF PERCEPTUAL INFORMATION

Perceptual organization.—Although difficult to analyze, perceptual organization has always been a provocative topic. The standard illustrative example has been the ambiguous figure. Present interest is typified by observations of fluctuation in organization of clusters of equilateral triangles (6), and figure-ground reversals (88, 216). Fisher (88) described an ambiguous figure with three alternative organizations, and Attneave (6) observed at least as many possible organizations of triangle clusters.

The perception of depth in static 2-dimensional representations such as orthogonal projections of parallelepipeds has been subjected to an intriguing theoretical analysis by Attneave & Frost (7). They took seriously the Gestalt notion that a figural organization would be as "good as prevailing conditions allowed." If the pictorial cues permit, a subject should see the representation as the simplest possible form, a parallelepiped, in its uniquely specifiable orientation. However, if some of the pictorial cues are already of maximum simplicity in the picture plane, they will not correspond to the maximally simple form, the parallelepiped, in its unique orientation in 3 dimensions. For example, if edges are all of equal length in the picture plane, they would not be in the 3-dimensional object. A subject judging the orientation of the edges with respect to the picture plane might compromise between seeing them as flat in the picture plane and seeing them in the orientation specified by the other pictorial cues. This analysis was confirmed by asking S s to match the orientation of the edges of such figures when different numbers of features were of maximum simplicity in the picture plane. The fewer features which were simple in the picture plane, the more nearly the slant judgment matched the uniquely specified orientation in 3 dimensions.

There are many instances of perception being organized in a coherent spatial framework. Numerous studies have shown marked effect of spatial orientation on perception (8, 9, 13, 159-161, 248, 308). Attneave (8, 9) showed that reaction times for identification (by label) of horizontal and vertical lines was faster than for oblique lines. The physical orientation was the determining factor, because the difference remained when *Ss* tilted their heads 45°. However, it was shown that *Ss* could give faster reaction times to retinally horizontal and vertical lines if they were instructed to pay attention to retinal orientation during identification training.

Recent studies have demonstrated another type of perceptual organization characterized by systemic functioning in complex perceptual and perceptual-motor tasks. Schneider (246) reported evidence for the existence in hamsters of two visual mechanisms normally confounded in animal discrimination tasks. One, mediated by the superior colliculus, is responsible for object localization, and the other, mediated by the visual cortex, is responsible for object identification. Rekosh & Freedman (228) argued for the integrated functioning of human directional systems on the basis of distortion experiments. During adaptation to visual distortion, while errors in visual localization decrease, there develop errors in the undisturbed auditory localization system.

Coding.—This term is used descriptively with two different meanings in perceptual studies. One use of the term has to do with identifiability of a stimulus (18, 41). Bindra, Donderi & Nishisato (18) consider a stimulus to be codable if it can be categorized absolutely, i.e. without reference to another stimulus. For example, colors and letters are codable and tone pitch and line lengths are noncodable. They showed that in same-different judgments codable stimuli require shorter reaction time for same judgments than for different judgments, while noncodable stimuli require longer reaction times for same judgments.

The other meaning of coding refers to restructuring of the stimulus during perception (81, 170, 174, 284). The use of this concept emphasizes the change in structure and the decrease in number of elements between stimulus and percept. Erikson (81) points out, for example, that to explain color vision it was not simply possible to extend the law of specific nerve energies because there were not enough neurons for one-to-one specification of colors all over the retina. A theory such as Thomas Young's was required with a few broadly tuned receptor types whose different patterns of activity specify different colors. The same situation obtains for other nontopographically organized sensory systems, e.g. temperature and taste.

Vitz & Todd (284) propose a model for the perceptual processing of sequential stimuli consisting of series of binary or trinary elements, e.g. abcaabbaab. According to this model, the elements are encoded into increasingly larger units until the whole sequence itself becomes an element. An uncertainty measure can be calculated for the sequence at each level of en-

coding. The sum of these measures agrees well with complexity judgments of such sequences.

The perception of forms and patterns is related to coding processes, especially of the second type. A major problem continues to be specification of the features perceived. In much recent work, randomly generated shapes are correlated with discrimination behavior and judgments of complexity, similarity, etc. (3, 15, 27, 28, 192, 239, 267). Behrman & Brown (15) obtained similarity judgments for pairs of random shapes presented tachistoscopically. Multidimensional scaling analyses suggested dimensions of compactness, jaggedness, and elongation as the basis of the judgments. These same dimensions formed the basis of discrimination latency in a related study by Brown & Andrews (27). Stenson (267) analyzed similarity judgments with somewhat different shapes and obtained dimensions characterized as complexity, curvature, curvature dispersion, and straight-length dispersion.

SELECTION OF PERCEPTUAL INFORMATION

How Ss choose and restrict the perceptual information impinging on them is the problem of selective attention. An immense amount of research has been reported on this topic. Here it will only be possible to touch on studies relevant to types of selectivity and changes of selectivity over time.

Types of selectivity.—Triesman (279) developed an elegant taxonomy of types of selectivity in perception. She suggests a dichotomy of studies addressing: (a) how well attention can be divided, or (b) how well attention can be focused. For either of these questions, selectivity can operate at the level of input, stimulus dimension to be analyzed, and target or particular stimulus value. Input selection refers to restriction of sensory data for further analysis on the basis of some property, e.g. modality, voice quality, and spatial position. Dimension selectivity refers to a choice of stimulus property to analyze. Target selectivity refers to choice or restriction of a particular end result of perception. For example, one might attend to the size (dimension) of visual stimuli (input) so as to look specifically for a stimulus of a particular size (target selection). Triesman suggests that division of attention between inputs or between targets is difficult, while division of attention between dimensions is relatively easy. Focusing of attention reciprocally appears to be relatively easy for inputs or targets and relatively difficult for stimulus dimensions.

Many questions asked about selective attention fit this taxonomy; representative and illustrative studies will be cited. Division of attention among targets was investigated by Ueno (280). Searching for any one of one, two, or three target numbers in a square array of digits was equally efficient. An exponential parallel processing model fits the data of time required for first detection. The result agrees with Triesman's summary that latency may not change as a function of competing targets, but that accuracy does decrease.

Focus of attention to a single input can be illustrated by a study of

Grindley & Townsend (124), in which Ss were asked to maintain fixation at a point and identify a stimulus presented peripherally in one of several loci. Foreknowledge as to where the target would appear resulted in improved identification, but only when competing dummy targets appeared in other areas. Focusing on input from a specific spatial area was possible, but only when other areas had noise, not when they were empty.

A study by Yonas & Gibson (103, pp. 142-143) illustrates the focus of attention on particular targets. Subjects were presented letters one at a time on a screen with instructions to identify a particular target letter or letters. Identifying a single target letter was faster than identifying any one of three target letters. However, if the three target letters could be identified by detection of a single distinctive feature, such as the presence of a diagonal line, performance gradually approached that for single letters.

Attention to stimulus dimensions has been the focus of especially interesting recent work. Lindsay, Taylor & Forbes (175) compared Ss' ability to discriminate stimuli varying in one, two, or four dimensions. The stimuli consisted of a combined visual display and auditory display, and the dimensions were horizontal and vertical position and pitch and loudness. The combination was presented twice on each trial and one, two, or four stimulus dimensions changed from the first to second presentation. Prior to each trial, S was instructed as to which dimension(s) to attend, but was only required to report on one. Mean detectability per dimension decreased with increasing number of dimensions regardless of whether multiple dimensions were in the same or different modalities. Thus division of attention between modalities was no harder than between dimensions. There was suggestion of a constant multichannel processing capacity which is divided with some loss among several dimensions. The processing could be sequential with rapid switching of attention or parallel with reduced capacity per dimension.

Egeth & Pachella (71) investigated a decrement in focus on one dimension in the context of a multidimensional task. Such distraction effects could be determined by comparing judgments of stimuli varying in one dimension with judgments of only one dimension for stimuli varying in several dimensions. Varying color exerted a distracting effect on judgments of size and shape, but these dimensions did not distract color judgments. In another experiment, an interference effect was found in which judgments alone on dimension (salinity) showed a decrement in the presence of a high intensity value of a second dimension (sweetness). A decrement in identification along one dimension, when several dimensions were judged, was shown not to depend on stimulus duration, and from this it is concluded that dimensions were processed in a partially parallel fashion.

Slak (256) asked if it is possible to predict the information processing capacity in multiple dimension discrimination if the single dimension discrimination capacities are known. He proposed a Euclidian distance measure: total information capacity in a multiple dimension discrimination is equal to the square root of the sum of squares of the single dimension capacities. His predictions fit reasonably well the existent data in the literature.

The most popular example of difficulty of focusing on single dimensions is the Stroop phenomenon, i.e. the difficulty in naming colors when the colored stimuli are printed names of other colors (70, 223, 225, 252). Triesman (279) suggested the difficulty may either be response interference or interference from superfluous stimulation, if the analysis of certain dimensions is obligatory. Egeth, Blecker & Kamlet (70), however, demonstrated that if S's task was to make same-different judgments about pairs of color words, rather than color identification, the traditional interference is not obtained. Interference was again found with same-different judgments of colors when the colored stimuli were the printed words *same* and *different*. Thus it would seem that when the stimuli elicit competing verbal responses, interference is obtained, but when the stimuli elicit verbal responses irrelevant to the judgment task, no interference occurs.

Several variations of the Stroop effect with different stimulus materials have been reported (200, 250, 303). Morton (200) asked Ss to sort cards according to the number of symbols on them. He found that if the symbols were digits incongruous with the number of symbols, there was marked interference. Indeed, there was interference from other nameable stimuli. He proposed a model in which the interference occurs after the sensory input (since auditory digits also interfere with visual sorting) but before the level of the sorting response.

Since Broadbent's early work, filter models of perceptual selectivity have been popular. A recent example of such a study is one by Wilding & Underwood (306). While shadowing a message presented to the right ear, Ss monitored a message in the left ear, tapping at occurrences of a particular vowel sound, a particular word, or exemplars of a particular concept. In fact, the actual targets were the same for all three conditions in the monitored message. For example, *steel* could be an occurrence of the vowel *ee*, the word *steal*, and an instance of a metal. The Ss monitoring for vowel sounds detected most targets, while those monitoring for a concept detected fewest. The present results are consistent with earlier work suggesting that filtering of unattended messages occurs at least partially after physical analysis, but before semantic analysis of the message. Filter-like selectivity has also been shown by Greenberg (120, 121) in experiments in which Ss listen for pure tone signals against a background of wide-band noise. Their results strongly imply actual attenuation of sensory information. However, Norman (205) has proposed a model of attention and memory without attenuation of sensory information. All sensory information automatically reaches a storage system and selection takes place subsequently.

The most obvious and natural way to investigate perceptual selectivity has always been examination of eye movements during visual tasks. The technology of eye-movement recording has been most intensively developed by Mackworth (184). Eye movements have been related to a variety of other tasks; chess playing (253), imagining forms (231), and copying pictures (201). One systematic investigation of eye movements in search tasks is that of Gould & Dill (118). The task was to find copies of a standard target in an

array. The standard was in the center of the array and the comparison stimuli in eight positions around it. A surprising result was that the size of the stimulus matrices (about 1° to 3°) did not affect number, pattern, or duration of eye fixations. The number of elements in a stimulus was related to number of fixations across all stimulus sizes. As the number of elements increased, number of fixations at first increased, then decreased. Duration of fixation to the standard stimulus was related to its *absolute* physical characteristics, but duration of fixation to comparison stimulus was related to *relative* physical measures between standards and comparisons.

The fact that information during one fixation can be received over a relatively wide spatial area is a difficulty with the use of eye movement recording. Another approach has been to infer search processes from changes in reaction time or latency and accuracy as a function of differences in the display. Many aspects of search processes have been so investigated: relation between ratings of information value and accuracy (220), relation to observing responses (48), distinction between detection and recognition (135, 203), rate of processing with sequential input (80). A study by Atkinson, Holmgren & Juola (5) is typical of this type. Subjects were required to report the presence, or absence, of a target letter in a line of letters varying in length from 1 to 5 letters. Latency increased as a function of number of letters at the same rate for both positive and negative responses. This result and the fact that latency does not change as a function of the lateral position of a letter seems to preclude a serial search process which terminates upon detection of a target. Serial exhaustive or parallel search processes are still consistent with the data.

Changes in selectivity.—Studies of vigilance typically find decreases in target detection with prolonged attention. Mackworth (183) proposed that such changes are due to two habituation mechanisms. The first is habituation of a general arousal or orienting response, resulting in decreased sensitivity. This occurs especially when the background events of the target signals occur at a very high rate or are continuous. The second is habituation of evoked potentials elicited by the specific background events, and it occurs mainly at relatively slow background event rates. This results in a reduced number of responses, both to the signals and to the background events. (The change in positive responses to both signals and noise represents a change of criterion β in signal detectability theory.)

An example of the first type of habituation can be found in a study by Hatfield & Soderquist (129). During a 90-min vigilance session, Ss listened to 5-sec pulses of white noise occurring every 2.5 sec. The target signals, which occurred on the average of one per minute, were pulses 1.6 db louder than the background pulses. Sensitivity, as measured by d' of signal detectability theory, decreased within sessions. However, in subsequent vigilance sessions, d' increased at first and then stabilized. An example of the second kind of habituation is from a study by Davenport (51). He asked Ss to detect

either low intensity auditory signals, or vibrotactile signals, or a combination of both. The signals occurred on the average of 1.4 min apart during 70-min vigils. During the course of the vigil, both the number of correct detections and the false positives decreased for the auditory and vibrotactile signals. However, no significant change occurred for the combination signals.

A variety of other vigilance studies have been conducted investigating such variables as exposures to signals (50), signal rate (176), payoff (52, 272), prior stimulation (148), social context (157), motivational set (177), and individual differences (53).

Gibson (103) has proposed that changes in perceptual selectivity are the basis of perceptual learning. She emphasizes abstraction, filtering, and overt orienting behavior as the mechanisms of such change. Abstraction, for example, is the discovery of an invariant among changes of objects and events. In the Yonas and Gibson experiment mentioned previously (103, pp. 142-143), Ss became able to detect any of three targets as rapidly as one, presumably by abstracting a common *distinctive feature*. In a relevant experiment by Aiken (2), Ss trained to discriminate auditory signals differing in frequency or duration showed as much transfer to different signals with the same *dimensions of difference* as they did to similar signals with new kinds of differences.

MOTOR COMPONENTS IN PERCEPTION

One important development of recent years has been the accumulation of evidence from a variety of sources about a motor component in perception. The evidence is more sophisticated than that for the classical motor theories of perception. In concluding this review some of the relevant work will be noted.

Some studies showing the effect of eye position control or registration on perception have already been noted (56, 130, 149, 212, 218, 257, 293). Matin, Matin & Pearce (191) reported a study of the perceived direction of light flashes during saccadic eye movements. If the direction is perceived accurately, it means the visual system takes into account the changing position of the eye. One alternative is that the direction might be perceived as if the eye had not moved. The results indicated only a partial compensation for the changed position of the eye, apparently based on the timing of the flash rather than on the actual eye position. Such compensatory results can be interpreted in terms of an "outflow" model, originally suggested by Helmholtz, that the visual system discounts image movement which is consequent upon voluntary (or *willed*) eye movement. A corollary of this model is that a stable image during eye movement will be perceived as a target in motion. Mack & Bachant (181) found a high correlation between perceived motion of an afterimage and eye movements.

That motor outflow to the eye can affect perception is also supported by evidence that visual sensitivity decreases during saccadic eye movements. In fact, thresholds are raised just prior to the beginning of eye movements (285).

However, Richards (232, 233) suggests an alternative explanation in terms of retinal activity produced by shearing forces from eye movement acceleration. The inhibitory effect prior to onset of eye movement may be only apparent—caused by the temporal delay of retinal processing itself.

One form of outflow model is that copies of efferent commands to muscles are sent elsewhere in the nervous system to be used for monitoring feedback and for other control functions. Steinbach (265, 266) demonstrated that eye tracking of the moving hand was considerably better if the hand movements were actively rather than passively made, suggesting that efference to hand movement is available for oculomotor control. He also showed that the important efferent information is about parameters of motion—velocity and acceleration rather than about target position.

In contrast with the view that perception is mediated by the actual outflow of muscle command information, Festinger et al (83) propose that perception (of size and shape) may be mediated by "efferent readiness." For example, if a retinally curved line elicits a tendency or readiness to make a straight eye movement, the line will be perceived as straight. This mechanism is used to explain adaptation to curvature distortion produced by prisms. More recently, Festinger, White & Allyn (84) have applied this mechanism to the explanation of the decrease in magnitude of the Müller-Lyer illusion with prolonged viewing. In scanning the illusion figure, Ss make larger saccades to the perceptually longer rather than shorter side. After a period of scanning the figure, the magnitude of illusion is less as is the eye movement difference. No such change occurred for Ss who fixated the figure without eye movements. The initial illusion may arise from a readiness to make different size eye movements in scanning across the two sides. With repeated exposure a new readiness is learned if Ss inspect the figure with eye movements.

The final evidence for motor involvement in perception comes from research in speech perception. Liberman et al (174) reviewed the research on speech perception from the Haskins Laboratory, making a convincing argument that at some stage of speech perception acoustic information is encoded in motor or articulation units. A basic thesis is that when differences in acoustic signal do not correspond with differences in articulation patterns, perception follows articulation rather than the acoustic signal. An example is that /d/ has a radically different acoustic signal in /di/ and /du/. What is common is that the articulation tract is closed at approximately the same place during sound production. In other cases differences in acoustic signal are only heard when they correspond to differences in articulation. For example, /b, d, g/ are distinguished acoustically mainly by difference in the second formant. The acoustic signal can be continuously varied, but small differences are detected only at phoneme boundaries where articulation changes and not within phoneme categories.

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TEST THEORY¹

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1. INTRODUCTION

This review covers the literature of test theory, test construction, and selected applied topics for the period January 1966 through December 1969, plus some material known to be in press. Because these 4 years span an interval of remarkable growth in research and publication in areas central to the field, space limitations have led us to exclude some of the more peripheral topics which have appeared in earlier reviews of test theory. Factor analysis, included in Tucker's (121) review, is omitted on the presumption that it will be treated elsewhere; fundamental measurement, treated by Keats (44), is assumed to belong under scaling (Zinnes 137); and personality assessment (Fiske & Pearson 29) is left to substantive reviews. The present review is thus delimited to test theory as *it has developed around problems of measuring ability and attainment* by means of objective tests. We justify this somewhat narrow focus on the grounds that recent progress in this area can be expected to have a major impact on all of psychological measurement. Future reviewers will undoubtedly have an opportunity to record the proliferation of these developments in other areas of application.

2. GENERAL TEST THEORY

The signal event in the review period is, of course, the appearance of Lord & Novick's (74) *Statistical Theories of Mental Test Scores*. For the first time, this volume brings under one cover the accumulated classical test theory, previously best represented in Gulliksen's (33) *Theory of Mental Tests*, and the growing body of modern test theory. Although the modern theory has its roots in Thurstone's (117) attempts to synthesize psychophysics and test theory, no important progress was made until the subject was cultivated from a more statistical point of view by Lawley (59), Lord (65), Rasch (95), Birnbaum (8), and others (see Solomon 111). Lord & Novick provide the only comprehensive account presently available of contemporary work in this field.

Significantly, the classical and modern approaches are roughly equally

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represented in Lord & Novick, a precedent which one would hope will be followed by subsequent texts and by graduate courses in the subject. Classical theory, which may be briefly described as an application of linear least squares estimation and analysis of variance along the lines of traditional error analysis, remains indispensable in the study of psychological measurement, but it cannot do the whole job either in theory or practice. Classical theory regards the score computed from a test as a continuous variable whose distribution is described adequately by a fixed expectation and variance; whereas actually, in fact, the score is in most cases a function of dichotomous or polychotomous variables and has statistical properties far more interesting and complex than classical theory envisions. The classical model is most plausible when a test consists of a large number of items which are homogeneous in difficulty and discriminating power relative to the presumed subject population. Unfortunately, much of psychological testing is carried out with tests which depart widely from this ideal. Modern theory, beginning from less restrictive and thus more realistic assumptions, provides methods of trait estimation and error assessment which can be justified in a far wider range of practical work.

While Lord & Novick's book is a great step forward in bringing classical and modern test theory into a better balance, in many ways it does not go far enough. The chapters on classical theory are an encyclopedic attempt to codify and make rigorous many different conceptions and interpretations of the classical model. This material is of inestimable value as a reference source, but pedagogically it is less successful because the student is uncertain where the core of the material lies. In teaching from this part of the book, one feels the need to supply some framework on which to develop the results—a framework which, in our opinion, might have been supplied by the linear mixed and random effects model for analysis of variance. Existing theory for this model provides a unified treatment of sampling from finite, infinite, or stratified universes, of estimating means and variance components, and of expressing precision in terms of variously defined intraclass correlation coefficients, etc. Unfortunately, this approach, which could have tied together the entire first three parts of the book, is assigned a single short section in Chapter 7. Some reworking of this material with pedagogical considerations in mind would definitely be welcome.

Among the chapters in Lord & Novick's book which are devoted to modern test theory, those which develop the normal ogive and logistic latent trait models will undoubtedly have the most immediate impact on mental testing practice. Because the authors (here including Birnbaum) are cautious in making claims for their results, the reader may fail to recognize the wealth of new applications which these models hold in store. The most important advantage is that, given a set of calibrated items which fit a latent trait model, one may calculate scores on the same scale from responses to any subset of items. This means, in particular, that alternative or partial forms

of a test may be scored on a common scale without resort to renorming, and that comparable scores can be obtained when, as often happens, all items are not administered to all subjects. It also makes possible the rigorous implementation of sequential item testing, since at any stage a score for the subject and the standard error of the score may be computed on the basis of items thus far presented. Thus, in a computer assisted set-up, a provisional estimate of the subject's ability can be calculated after each item is presented, an additional item can be selected so as to give the largest increment of information to the estimate, and the process continued in this way until the standard error of the ability estimate becomes acceptably small. This type of testing is now technologically possible, and promises substantial reductions in testing time for equal or better precision of measurement. It follows from the fact that the item, and not the test, may now be regarded as the basic unit of measurement.

The latent trait models also offer the advantage of measurement on a scale with a well-defined metric. This means that the ability estimates, or any one-to-one transformation of them which theory may dictate, may be used to measure growth and change. Traditional methods of scoring tests are markedly deficient in this respect and have hindered the study of growth in ability and in other latent traits.

Lord & Novick's book does not elaborate on these possibilities to any extent but the appropriate theoretical treatment, including the information analysis for optimal choice of items, is presented. Actually, only a few substantive applications of the latent traits model had been attempted at the time the book was written (see Lord & Novick 74, p. 370). However, considerable progress in this direction has been made since then by Lord (68, 70, 71, 73) and others (4, 6, 9, 11, 13, 16, 51, 77, 91, 92, 105, 114, 131, 132), and the computer programs necessary for practical work are beginning to become available (see Section 7 of this review).

Recognizing perhaps the shortcomings of the classical test theory model, Lord & Novick devoted some space to the exposition of so-called strong true score models, such as the binomial model which permits true score and error to be correlated. Some of this work is alluded to in Section 4 of this review.

Other books published during the review period in the general area of test theory have been less ambitious and, in various ways, less successful. Magnusson's (75) book presents test theory at an elementary and rather diluted level. Some idea of the book's aspirations may be gained from this passage from the foreword: "experience has shown that even relatively simple statistical notation and derivations present difficulties for many students of test theory."

Horst's (39) book offers a general coverage of psychological measurement within which test theory finds a place. The test theory material is in fact rather idiosyncratic: Lord is barely mentioned, yet prediction problems are given extensive treatment. Horst obviously does not care for the item sampl-

ing approach to test construction, sharing Loevinger's (64) reservations about the propriety of defining item populations, and preferring to rely on cut-and-try methods.

Like Horst's volume, Nunnally's (86) book ranges well beyond test theory with only about a fifth of the book given over to the subject. By Nunnally's own admission, the book is for the average reader and he anticipates that the treatment will be too discursive for the mathematically inclined, which turns out to be the case. There is an extended discussion of the item characteristic curve which places it in relation to psychophysical scaling models. However, readers should be warned that Nunnally (86, p. 73) is not overly fastidious about the distinction between true score and latent trait or between item-true-score regression, item-latent-trait regression, and true-score-latent trait regression (see Lord & Novick 74, pp. 386-89). Apart from this nontechnical excursion into latent trait theory, the book does not deal with modern test theory except for a section on generalizability theory, which Nunnally promotes with fervor. Otherwise, the book is a thorough treatment of various salient topics—guessing, correlation coefficients, factor analysis, item analysis—accompanied by do's and don'ts for the benefit of the practical worker.

Rozeboom's (102) book is unique, not only because of its content, but also because of the unusual type-set used for some of the symbols and the somewhat bizarre language (e.g. "the penetrance of x into any neuterspace S_0 "). In fact, to be properly appreciated, the book must be approached through Rozeboom's own brand of factor analysis which leans heavily on the centroid solution. From a conventional point of view, the book is most useful when it discusses test reliability and its relationship to factor analysis. Rozeboom makes some pointed remarks about correlated measurement error and, in general, challenges assumptions of the classical model which are often taken for granted. For example, he observes (102, p. 427) that factor analysis of inter-item covariances often fails to support even the assumption of a single common factor, much less that the variance attributable to this factor is the same for all test items. By conceiving of the test as a linear composite of (possibly) heterogeneous items, and investigating their factorial composition, he arrives at a multifactorial model of test scores in which reliability becomes a function of the factor loadings on the principal factors (see also LaForge 58).

Rozeboom has called attention to a rather interesting area of enquiry which no one seems anxious to develop, namely, a multidimensional theory of item response. One can envision the possibility of creating models for items which fall on two or more latent dimensions and estimating, by maximum likelihood, the loadings of the items on each. Lord & Novick (74, pp. 359-62) introduce the generalized latent trait model for k -dimensional latent space but then specialize it to the usual one-dimensional case.

Reviewing books in the area of test theory, one is struck by the need for a uniform system of notation in this field. Perhaps it is time for psychometricians to put their terminology and notation in order as the statis-

ticians have done (Halperin, Hartley & Hoel 35). Lord & Novick have, in fact, given a lead by conforming in large part to this latter group's recommendations.

3. CLASSICAL TEST THEORY

Specific and generic true scores.—Classical test theory was reworked by Novick (80) and later Lord & Novick (74) with the aim of producing a statistically rigorous model on the weakest possible assumptions. By conceiving of observed score, true score, and error score as random variables, Novick (80) was able to rederive the basic axioms of classical test theory and show, in particular, that the true score and error random variables are uncorrelated by construction rather than by definition. This *specific true score* may be regarded as the expected value of a person's observed score on very many replications with parallel tests and should not be confused with *generic* true score, which is the expectation over a population of different test forms which are only parallel in the sense they arise from random sampling of the item population. We will return to the concept of generic true score, which is a radical departure from classical test theory, in the ensuing sections on reliability and item sampling.

Objections to the classical model.—While many psychometricians have regarded the classical test theory model as at least a useful approximation to reality and have not bothered to challenge it, there has lately been some movement to broaden, modify, or jettison some of its assumptions. Zimmerman et al (123, 134–136) and Traub (118, 119) have argued that true score and error are bound to be negatively correlated in multiple choice tests. Their argument is based on the supposition that subjects with low true score tend to guess more than those with high true score, and manage to augment their authentic scores with spurious correct answers.² This supposition has some empirical support; Lord (66) found heteroscedasticity of error variance even within ability groups as well as non-normality of the distribution of errors. Lord & Novick (74, p. 233) attribute these results to the presence of floor and ceiling effects, i.e. scores on an n item test are bounded at 0 and n so that high scorers are relatively deprived of items to which they might apply partial knowledge.

The correlated true score-error case arises in somewhat disguised form in the so-called Platonic true score model (Klein & Cleary 45, 46). Platonic true score is the intrinsic true score when all systematic error is removed. As distinct from the operational version of true score as the expected value

² Although widely accepted, this argument may be more complicated than it appears. On the basis of a study of male college students, Kogan & Wallach (50) contended that degree of motivational disturbance (anxiety, defensiveness) in the test situation exerts an influence on the capacity of guessers to utilize the partial formation available to them. This conclusion, however, was restricted to tests of verbal ability.

of observed score, which they favor, Lord & Novick (44, p. 39) refer to the Platonic score as the mode of the propensity distribution.

It must be pointed out, however, that the question of whether or not to absorb biased error into true score is a matter which has been dealt with routinely by statisticians who have studied measurement error (Cochran 20). Following Cochran, we trace the following sequence of model development:

$$1. \quad 1. \quad x = t + e; E(e) = 0, Cov(t, e) = 0 \quad \text{[Classical model]}$$

$$2. \quad x = t + e_b + e_r; E(e_r) = 0, Cov(t, e_b) = 0, Cov(e_b, e_r) = 0$$

[Expanded classical model]

where e_b = biased error, e_r = residual error.

If attributed to guessing, e_b is sometimes called topastic error variance and e_r , scedastic error variance (Carroll 15).

$$3. \quad x = t + e_b + e_r; E(e_r) = 0, Cov(t, e_b) \neq 0 \quad \text{[“Platonic” model]}$$

$$4. \quad x = t^* + e_r; E(e_r) = 0, Cov(t^*, e_r) = 0$$

[Operational classical model]

where $t^* = t + e_b$.

Klein & Cleary (45) showed that for a single item, which is scored 0 or 1, there is bound to be negative covariance between true score and error, but were unaware that this result had been proved for binomial data some years previously by Hansen, Hurwitz & Bershad (36). Furthermore, there already exists in test theory a true score model—the binomial error model (Lord & Novick 74, Ch. 23)—which is explicitly founded on the premise that true score and error are not independent. This suggests that, as a general principle, it is not worth tinkering with the basic classical model strictly in order to preserve it, but rather to switch to a stronger true score theory when the occasion demands. There seems little to be gained from further prolonging the life of the Platonic true score as a concept in test theory.

A similar judgment applies when violation of the assumption that a subject's error is uncorrelated from item to item is suspected. Rozeboom (103), in an otherwise complementary review, belabored Lord & Novick for failing to account for various endogenous and exogenous factors which allegedly exert a continuing effect on a subject's test performance. Little is known about these effects, although presumably they will contribute to autoregression effects after biased error has been removed. Short of developing a serial correlation model which includes these effects, the tester could minimize their impact by using random arrangements of items in the tests taken by different subjects. The problem of deciding between the classical model and some stronger alternative model has been discussed from a Bayesian point of view by Hill (37). He was concerned with the problem of negative estimates of true score variance, which, in a one-way

ANOVA model, result when the within-subject mean square becomes too large relative to the between-subject mean square. By placing prior probabilities on (a) the ordinary uncorrelated errors model and (b) a model where successive errors were taken to be negatively correlated, he succeeded in showing that, as the within-person sum of squares becomes large, the probability that the alternative model holds tends to one. Such an approach could presumably be taken in the problem discussed above, except that errors would have to be assumed positively correlated. Considerations of this kind underline the advantage of analyzing test performance within the framework of a variance components model, where one is in a position to modify the true score model as necessary, whether or not one utilizes prior probabilities.

4. STRONG TRUE SCORE THEORY

Empirical Bayes estimation.—Departures from weak true score theory of the kind described in the earlier sections will no doubt become more common as the consequences of better defined models are explored. Three chapters of Lord & Novick (74) are given over to strong true score theory, the first dealing with Rasch's Poisson model for misreadings (see Section 6 below), the second with the binomial error model mentioned above, and the third with one of Lord's continuing interests, that of inferring true score distributions from observed score distributions. As Lord has pointed out (69), if this inferential problem can be solved, the complete bivariate distribution of true score and observed score can be estimated, including the regression of true score on observed score. Furthermore, the item characteristic curve for particular items can be estimated without strong prior assumptions as to its mathematical form (Lord 72).

It is evident that weak true score theory is not adequate for the task, since no inference about the distribution of true scores from observed scores can be made without specifying the distribution of number correct as a function of true score. If this strong assumption is made, then the problem becomes one of estimating a prior distribution of ability among a population of subjects from a random sample of examinees. This is an *empirical Bayes estimation* problem. The observed scores can be regarded as independent realizations of a random variable x whose probability distribution is the mixture

$$p(x) = \int p(x|\xi)dg(\xi)$$

where $g(\xi)$ is the true score distribution to be inferred and $p(x)$ is known.

Lord (72) assumed that $p(x|\xi)$ is a compound binomial distribution and that $dg(\xi)$ is a four-parameter beta density. The solution for $\hat{g}(\xi)$ is obtained via a rather complicated procedure which involves smoothing of the curve, followed by substitution of $d\hat{g}(\xi)$ in the equation above, after which $\hat{p}(x)$

is checked against $p(x)$ for goodness of fit. Lord reported difficulty with the smoothing process but subsequent papers in the statistical literature (60, 104) may be of some help in this respect, depending on the form of the prior density. Although Lord (72) obtains reasonably good fits for $p(x)$, Lord & Novick (74, p. 527) conclude that much work remains to be done on this problem from the point of view of statistical inference.

Bayesian estimation of true score.—Various Bayesian estimates of specific true score are realizable depending on the nature of the prior distributions ascribed to the true score and error. In a pioneering paper, Novick, Jackson & Thayer (83) have thoroughly studied the merits of the respective candidates. Because the subject is rather technical and there are so many different models to be discussed, we shall restrict ourselves to a few general remarks.

As we mentioned above, a disconcerting feature of the one-way components of variance model is the occasional negative estimate of true score variance. Bayesians (Hill 37; Novick, Jackson & Thayer 83) believe that the classical method falls down because it neglects the information about the error variance contained in the between-person sum of squares, choosing instead to estimate error only with within-person sum of squares. They also claim to have a way of coping with the problem. We have already seen how Hill (37) handles it; Novick, Jackson & Thayer (83) go about it a little differently, demonstrating that the Bayesian Structural Model, as they call it, supplies a posterior distribution of the error variance which does, in fact, depend on the between sum of squares as well as the replication sum of squares. They show that where the between mean squares is much less than the within, the modal estimate of error variance depends on both of them, and the estimate of the true score variance goes to zero, which is less uncomfortable than a negative estimate.

For estimating true score, Novick, Jackson & Thayer (83) describe several Bayesian methods. They find Lindley's (62) posterior modal estimate, based on an indifference prior (Novick 81), to have greatest utility from the standpoint of completeness and computation. They also contend that Bayesian methods are most effective when reliabilities of means over replications differ, as for example in the case of unequal replications. Although mental test data of this description would not ordinarily be expected to arise in conventional paper and pencil applications, it is precisely what one expects in individualized or tailored testing. This enables us to take advantage of an important Bayesian result. If n sets of observations are gathered consecutively, then the posterior distribution calculated after the i th set may be regarded as the prior for the $(i+1)$ th and so on, up to the n th, but the final posterior will be no different whatsoever from the one which would have resulted had all n sets of observations been collected and analyzed simultaneously. Accordingly, one is, in principle, in a position to improve the estimation of a person's true score by using relevant information gathered about fellow members of an appropriate reference group. Novick (82) has

advocated that such knowledge should be utilized in tailored testing on the grounds that if you know little or nothing about a person's abilities, your best initial guide is the mean score of other people like him. As data about him accumulates, there is less and less need to rely on group performance.

5. LATENT TRAIT THEORY

The normal ogive and logistic models.—Recent research in latent trait theory is confined largely to the normal ogive model of Lawley (59) and Lord (65), and the logistic models of Rasch (95) and Birnbaum (8). Developments in this field up to 1968 are reviewed in Lord & Novick (74, Ch. 16). Subsequent works have dealt with (a) estimation problems and (b) generalization of the models.

At the time Lord and Novick were writing, the problem of estimating parameters of the normal ogive and logistic models was not sufficiently resolved to be treated at length. However, they did discuss two possible approaches to estimation, the first of which they applied to the normal ogive model, and the second to Birnbaum's logistic model. Loosely speaking, the first of these is a correlation method and the second a regression method, although in the latter it is a latent rather than an observed variable which is regressed upon.

If it is assumed that the calibration sample is drawn randomly from a population in which ability is normally distributed, and if the normal ogive model for the item characteristic function holds and there is no guessing, then a correlational method may be used to estimate item discriminating power. Under these assumptions, the first factor loadings of the matrix of inter-item tetrachoric correlation coefficients³ are consistent estimators of the item reliability indices. Similarly, item difficulty parameters are consistently estimated by the normal deviates corresponding to percent of correct responses in the sample. A criterion of goodness-of-fit is provided by the extent to which the matrix of tetrachoric correlation coefficients approximates a single-factor structure. This approach has been successfully used in an applied study by Indow & Samejima (41).

Bock & Lieberman (13) have also shown that, for small numbers of items, a direct maximum likelihood solution for the discriminating and difficulty parameters can be achieved under the same assumptions. For the sample they studied, the maximum likelihood approach and the method based on tetrachoric correlations gave closely similar results. They also showed that guessing effects could be allowed for by incorporating Carroll's (15) correction of the tetrachoric correlation for guessing, and correcting the sample item difficulties for guessing.

If the subjects who are to be used to calibrate the items are an arbitrary

³ Incidentally, Froemel (31) has shown that the commonly used computer routines for computing tetrachoric correlations can be quite inaccurate. He suggests the use of the more accurate routine listed in part 7 of this review.

collection of individuals rather than a sample from a population, a regression approach must be used in estimating the item parameters. The traditional method of regressing items on total test score is a heuristic application of this approach. It may, in fact, be regarded as the first stage of the solution suggested by Birnbaum (8) which involves simultaneous maximum likelihood estimation of item parameters and latent ability. In Birnbaum's procedure, the items are regressed on provisional maximum likelihood estimates of the subject's latent ability instead of on test score, and the procedure is iterated to convergence. Lord (68) has found that, for the logistic model, and including the guessing parameter, the procedure does in fact converge to reasonable estimates provided certain subjects and items for whom the estimates are indeterminate are eliminated. Kolakowski (51) has reported a similarly successful solution for the normal ogive model, and finds that the computer time required can be greatly reduced by grouping the subjects according to their provisionally estimated latent ability at each stage.

In view of the convenience of this *sample-free* method of item calibration, to use Wright's (131) term, the regression approach has a greater range of potential application than the correlational approach. However, it should be understood that the regression approach does not serve all of the purposes of the traditional population-centered item analysis. In the traditional approach, the calibration sample is assumed to represent a specified population, and item parameters and ability estimates obtained from it are normed with respect to the population. This gives an absolute meaning to the origin and unit of the estimates relative to the population. By contrast, the sample-free rationale employs an arbitrary origin and unit of scale, and only comparisons (contrasts or ratios) between items or subjects are meaningful. One cannot say that item A is "difficult," but only that item A is more difficult than item B at a specified level of ability. Similarly, only the relative ability of specific subjects can be expressed. Of course, if one wishes to give the regression solution a population reference, then he must have a sample, and may fix the origin and unit of scale by, say, setting the sample mean ability to zero and the sample variance to unity. Ability may then be expressed in standard scores and item difficulty as median liminal ability.

A valuable extension of the latent trait model to include graded scoring of the items has been introduced by Samejima (105). She assumes that the item responses are scored in successive categories, such as "incorrect," "partly correct," "correct." A familiar example is the scoring of the vocabulary test in the WAIS, where partial credit is given for less than complete definitions. She defines item characteristic functions for the graded categories by using dichotomous functions, all with the same slope, for responses in or below each category. Differences between successive dichotomous functions give the response probability for the included category as a function of ability. This is the same model which was used by Thurstone for scaling successive categories data (see Bock & Jones 12, Ch. 7) and by Aitchison & Silvey (1) for bioassay of graded data. Samejima obtains the conditions on

the dichotomous functions which are necessary and sufficient for the existence of unique maximum likelihood estimates of ability under the graded model and shows that the normal ogive and logistic functions satisfy these conditions. She also gives a definition of information⁴ per response which is suitable for the information analysis of items scored in multiple categories. Total information for a graded item, and thence for the test as a whole, is obtained by summing the per response information. She shows that the shift from dichotomous to graded scoring always increases information, provided all categories have nonzero response frequency.

Samejima does not consider estimation problems in any depth, and in her example merely uses average tetrachoric correlations between items (based on dichotomizing the responses at each grade) to obtain an estimate of the common discriminating power of each item by factor analysis. Normal deviates calculated from the cumulative proportion of responses in the successive categories estimate the difficulty parameters corresponding to category boundaries. An improvement in the case of the discriminating power estimate might be to compute an inter-item polychoric correlation coefficient for purposes of the factor analysis. A maximum likelihood solution for polychoric correlation, based on the work of Tallis (115), has been reported by Lieberman (61). It should also be possible to estimate the parameters of the graded model by the regression method described above for the dichotomous case, but no attempt in this direction has been reported thus far.

Baker & Gurland (6) proposed an extension of the normal ogive model for items scored in multiple *nominal* categories. Their model has the property, however, that the probability for each category goes to unity with increasing ability. Thus, the model is suitable for items such as those in the spatial subtest of the PMA (Primary Mental Abilities), where each alternative is marked if it agrees with the stem, but not for typical multiple choice items where the alternatives are mutually exclusive. A model suitable for the latter case has been proposed by Bock (11) based on his multivariate logistic model for multinomial response relations (Bock 10).

The Rasch model.—The Rasch model (95–97) is a special case of the logistic model in which the parameter for discriminating power is assumed the same for all items and is absorbed in the unit of scale of the ability estimate. Where the assumption of uniform discriminating power is appropriate, this model has the advantage of greater computational simplicity, chiefly because the test score (number right) is a sufficient statistic for the estimation of latent ability. This contrasts with Birnbaum's logistic model, where a weighted sum of the dichotomous item scores is the sufficient statistic.

The Rasch model is also simpler to interpret in that items may be com-

⁴ This is information in the Fisherian sense, i.e. the inverse of the asymptotic variance.

pared in difficulty without respect to a population or to level of ability. This is the case because the rectified regression lines of items on the latent trait are parallel and their intercepts characterize difficulty without respect to ability. In the general logistic model, item difficulty and ability interact so that overall item difficulty can be characterized only at specified abilities or, as is more common, by integrating over the population distribution of latent ability. Other properties of the Rasch model, such as the possibility of computing scores on a common metric from any subset of items, or estimating item parameters by regression methods which do not require sampling of a population of subjects, are shared by the other latent trait models.

Rasch's early parameter estimation procedures were, by his own admission (96, p. 104), rather primitive and some effort has been devoted to improving them. In this connection, Wright & Panchapakesan (132) and Panchapakesan (92) used maximum likelihood methods to secure simultaneous estimation of item and ability parameters. They also made explicit the information concept and introduced a test of goodness-of-fit of the model. Choppin (16) considered a more rudimentary estimation procedure, exploiting the property of the model that estimates of pairwise ratios of item parameters can be obtained independently of the subjects attempting the items. Andersen has generalized the model to the polychotomous response case and, we understand,⁵ is enriching the model in other ways, including, it seems, a Bayesian estimation procedure (3, 4). Empirical studies of the fit of the model have been reported by Wright (131), Fischer (28), Anderson, Kearney & Everett (5) and Mellenbergh (77), among others.

Although the Rasch model would not apply in the common circumstances where variation in item discriminating power is too large to be ignored, it is nevertheless convenient both from a practical and theoretical point of view when applied to item sets which have already been selected for uniform discriminating power. Thus, a prior item analysis with the more general models would be a good preliminary to the application of the Rasch model. Alternatively, deviations from the latter would suggest items which should be excluded from the analysis.

Bayesian models.—A prominent feature of the past 4 years of work in mental test theory has been the steady influx of Bayesian methods. [See Novick (82) for an overview of this movement.] In the context of latent trait theory, the Bayesian rationale, in which all variables are taken to be random, naturally accommodates the desire or need to make distributional assumptions about the parameters to be estimated, which we have seen is already explicit in the unconditional approach to estimation. For prior distributions which blend with the item response likelihood under Bayes theorem without increasing the parameter space (the natural conjugate property), the

⁵ Personal communication from B. D. Wright.

posterior distribution of ability retains the same form as the prior. This is true of the normal ogive (105) and the logistic response models (9, 105).

The degree of confidence one places in the form of the prior distribution is perhaps not an issue. All that matters, as Mosteller & Wallace (79) observe, is that one should be in the neighborhood of a distribution suggested by a fair amount of past experience. In other words, this almost nonchalant but realistic attitude toward the prior, which Cornfield (21) claims is shared by most Bayesians, amounts to nothing more radical than, as Cornfield (21) again notes, a rejection of the view that a given set of data yields a unique inference—a position which mental test theorists have long accepted.

When Bayes estimation is applied to latent trait models, a number of different ability estimators may be entertained. Samejima (105) discusses two possibilities. The first, which she calls a Bayes modal estimator, is simply the solution of the likelihood solution with the log of the prior density added on. (One may imagine the prior as comprising a series of hypothetical prior observations from the same process as the sample distribution.) Samejima (105) also noted that, because the prior density is not usually transformation-free, the Bayes modal estimator does not possess the transformation-free character of the ordinary maximum likelihood estimator, except when the transformation is linear. This reflects the fact that in Bayes estimation the commitment to the metric is in general made at the time the prior is chosen. The second estimator discussed by Samejima is the Bayes estimator, obtained by minimizing the expectation of the mean square error. A well-known result states that for a squared error loss function the Bayes estimator is the mean of the posterior distribution of θ (Krutchhoff 57). Samejima compared estimates produced by the two estimators and found very high agreement between them. However, the modal estimator is generally easier to compute than the least-square estimator and is often preferred in practical applications. The notion of information is, of course, retained in the Bayesian analysis as the inverse of the posterior variance or loss.

Sequential item testing.—Conventional group testing of subjects is, to some degree, inefficient. Very little is learned about bright examinees from their encounters with easy items, while information in the responses of weak examinees is seriously degraded if they guess answers to difficult items. One of the distinct advantages of the information analysis in latent trait theory is that it indicates the precise level of item difficulty which is optimal for testing a subject of any given ability. This is in sharp distinction to traditional reliability theory which treats precision of measurement as if it were independent of level of ability. Latent trait theory therefore raises the possibility of making testing more efficient in choosing items which are optimal at the subject's level of ability, as provisionally estimated from his responses to previous items. This procedure of fitting a test to an individual, Lord (70) has called tailored testing.

Two approaches to tailored testing have been proposed. In the first, a system of rules governing the testing strategy is mapped out ahead of time. Branching rules (correct answers lead to harder items, incorrect answers to easier items) and step sizes (how much should consecutive items differ in difficulty) are established and a scoring function is chosen; number right is not necessarily the best scoring procedure. (See Lord 70).

The first studies of tailored testing (18, 19, 63, and others cited there) were frankly empirical explorations of the effects of step sizes and scoring functions, as well as the possible advantages of choosing items in blocks instead of singly. The advantages of two-stage testing procedures, in which subjects are first given a routing test to classify them, followed by a finer grained measurement test, were also investigated.

These studies have certain methodological shortcomings to which Lord (70) has drawn attention. Correlating scores on a tailored test and an "equivalent" conventional test is of dubious value, since the extra information gleaned about certain individuals—the whole point of tailored testing—is lost when information is pooled in a group statistic. Standard reliability measures are also inappropriate since there is a plethora of possible item sequences which will seldom be duplicated.

These deficiencies were largely due to the absence of any theoretical basis for interpreting item response sequences, now remedied by Lord's (70) integration of tailored testing with the latent trait model. This makes possible a meaningful comparison of the relative efficiency of tailored tests to conventional tests in terms of information yielded about individuals at different ability levels. Another improvement is to shrink the step size as the tailored test converges on the examinee's ability. This procedure, known in the biometric literature as the Robbins-Monro process, was investigated in depth by Lord in a subsequent publication (73). Stocking (114) has also reported on the effectiveness of short (15 item) tailored tests, and Lord (71) has recently looked into two-stage testing, again from the point of view of latent trait theory. The general conclusion from these studies seems to be that while tailored tests cannot improve much on conventional tests at the median ability level, they offer advantages at the extremes, and hence are especially useful when subjects of wide-ranging ability are to be tested.

The second approach to tailored testing is to dispense altogether with prior structuring of item combinations and to perform the item selection during test administration. Such a procedure necessarily requires "real time" interaction with a computer, but with the advent of time-sharing systems, the technological possibility exists. The fact that this type of sequential item testing lends itself to a Bayesian formulation has been noted by Novick (82) and Stocking (114). They postulate a prior distribution of ability for each subject (rather than for the group) and choose items at each stage so as to minimize the posterior variance of his ability estimate, or alternatively, maximize the information gain. Owen (91) has shown that,

in the absence of guessing, the optimal item at each stage is the one with difficulty equal (or approximately equal) to the provisional Bayes estimate of ability (the posterior mean). [Compare the similar result in the non-Bayesian formulation (120).] If more than one item should satisfy this criterion, which is unlikely, the item with greatest discrimination is chosen. Owen's scheme deals only with dichotomous item responses, but clearly may be extended to graded and nominal responses via suitable latent trait models (Samejima 105, Bock 111).

A characteristic of Bayesian sequential item testing is the idea that a data-gathering process may be terminated or redirected at any time. Thus, we may view the subject's stay at the computer console as an opportunity for extracting from him as much relevant information as time and money allow. This might include personality and attitude oriented inquiries or any other information, in addition to ability, which can be predictive in a particular context. With a sufficiently ambitious information analysis system and a multivariate prediction function, it should be possible to trade off, within fixed time limits, better precision for some variables against a greater range of qualitatively different variables, with the aim of maximizing overall prediction [for a comparable procedure, see Jackson & Novick (42)]. It may also be possible to defeat subjects suspected of guessing⁶ by repeating questions in slightly different form.

6. OTHER TOPICS

Reliability theory.—In the study of test reliability there continue to be outstanding statistical problems, some connected with the original specific true score theory and others connected with the newer theory of generalizability.

In the first case, the widening of the concept of equivalence beyond strict parallelism to include τ -equivalent tests (equal true score variance but possibly different error variances) led to fresh results. Novick & Lewis (84) showed that coefficient α coincides with the reliability coefficient ρ , if and only if α is based upon essentially τ -equivalent parts of the total test. Otherwise α will be smaller than ρ . Hunter's (40) paper on the probabilistic foundations of coefficient α is relevant in this regard.

Other definitions of equivalence have been proposed. Kristof (54) derived maximum likelihood estimations of true score variance and error variance for tests under six different cases of equivalence with special attention given to τ -equivalent measurements. He found that the initial estimators of the error variance needed to start the iterative solution—using a least-squares routine—are almost as good as the maximum likelihood

⁶ Guessing ought to be less of a problem owing to the nature of the testing procedure, one consequence of which is a greater probability that partial information will be equalized relative to each ability level

estimations, and much easier to compute. He found the same to be true for a reliability estimate based on a ratio of the initial estimates of true and observed score variance. In subsequent papers, Kristof (55, 56) investigated the sampling properties of test reliability coefficients for yet another version of equivalence called "essential parallelism" where the equality of means requirement is dropped. A maximum likelihood ratio test of a point hypothesis concerning α and a method for constructing confidence intervals for α at any chosen level of confidence when a test has been split into just two parts, was given.

With a view to organizing these rather fragmented results, Jöreskog (43) proposed a model for *congeneric* test scores. If the test scores $x_1, x_2, \dots, x_n$ are congeneric, then any pair of true scores t_i and t_j correlate perfectly and there exists a random variable τ such that each t_i is linearly related to τ , i.e.

$$t_i = \mu_i + \beta_i \tau$$

When generalized to multivariate measurements, this leads to the basic factor analysis model with a common factor. Jöreskog, in fact, assumed a factor structure for true scores and tested the modified model against the congeneric criterion. He was also able to estimate variance components by a maximum likelihood method. All other models of equivalence can be derived as special cases of Jöreskog's model, and the possibility of specifying further true score and error score variance-covariance structures and estimating reliabilities is clearly implicit in the model.

As is well known, unless distributional assumptions are made, a ratio of unbiased variance estimates does not provide an unbiased estimate of a specific reliability coefficient, although it does supply a consistent estimate (Lord & Novick 74, p. 203). Various suggestions for curbing the bias are discussed in Lord & Novick (74, p. 202). Srivastava & Webster (112) considered various corrections in which item response is treated as a continuous variable, but they did not arrive at a markedly improved approximation to unbiasedness. In any case, where the number of subjects is large, the extent of bias becomes negligible.

Using an ANOVA approach, Maxwell (76) showed that, where no account is taken of topastic or biased error variance, the estimate of the specific reliability coefficient will be biased (that is, more biased than usual) and in the most commonly occurring situations, i.e. when errors are positively correlated, will be an overestimate. Zimmerman & Williams (123, 135) made a similar observation and derived Spearman-Brown and split-half of KR-20 coefficients with appropriate correction factors. Zimmerman and his colleagues (123, 135, 136) have also shown that these revised formulas are unaffected by violation of the assumption that true score and error are uncorrelated.

Item sampling and generalizability theory.—Item sampling and generalizability theory interpenetrate to such an extent that it is difficult to draw hard and fast lines between them. The latter theory, formulated by Cronbach, Rajaratnam & Gleser (23), regards a collection of items or a test as a random sample from an infinite universe of items, in recognition of the fact that a set of items, put together according to rule-of-thumb criteria, offers no inferential scope or substantive meaning. From this conceptualization follows the notions of *generic* true score, the expected score over all items in the population, and generic reliability, the reliability of a whole family of randomly parallel test forms. Apart from developing these ideas, the generalizability school was also responsible for the notion of stratified random sampling of items—stratification according to content or difficulty or conceivably some other criterion—and the important distinction between a matched item sampling design (everyone gets the same items) and an unmatched design (every person gets a different random sample of items).

Lord (67), working independently, cultivated his item sampling model along rather different lines, concentrating on the estimation of moments of the population distribution of ability, and dealing in depth with the situation where both people and items are randomly sampled (so-called matrix or item-person sampling). Lord also provided theory for the more realistic circumstances where the populations of people and items are finite (Lord & Novick 74, Ch. 11). He has now sufficiently elaborated the item sampling model to justify his claim that it constitutes a mental test theory in its own right.⁷

While generalizability and item sampling theory are quite compatible, empirical research in each area has tended to remain distinct, except, that is, for Osburn's work, which has served as a bridge between the two. In an analytic paper, Osburn (87) showed that matched item sampling designs are superior for measuring individual differences, whereas unmatched designs are best for estimating means and differences in means. The former result hinges on the fact that the covariance between persons in the matched case is subtracted from the variances, thus reducing the variance of the differences and rendering the design more efficient. The latter depends basically on the fact that the standard error of the group mean score is always smaller when unmatched data are collected since this time the covariance between persons is nil (see also Lord & Novick 74, pp. 258–59). For reports of empirical studies comparing the efficacy of matched and unmatched item sampling designs, for both stratified and unstratified sampling, see Shoemaker & Osburn (107) and Osburn (89).

The promised utility and experimental appeal of item sampling methods

⁷ Unpublished address delivered at a symposium on Item Sampling held under the joint auspices of the American Educational Research Association and the National Council on Measurement in Education, Minneapolis, March 2–6, 1970.

has brought forth a crop of empirical studies (for a bibliography, see Knapp 49). Most are only partial repetitions of Lord's (67) original study; only two attempted to reproduce the frequency distribution of test scores and several only estimated population means. A few studies have employed *a priori* item sampling schemes but most have been *ex post facto* in nature, with the usual assumptions about item context. In any case, Lord's early findings have tended to be confirmed; for estimation of the mean, item sampling does as well as or better than examinee sampling, but for variance estimation the position is not so clear-cut.

Of the more theoretically inclined studies, Sirotnik (110) took the matrix sampling model and treated it as a two-way random effects ANOVA model. To his consternation, he encountered negative estimates of true score variance, probably due to the items he sampled from not being sufficiently homogeneous. Knapp (48) more or less avoided this problem by designing his study in such a way that each item appeared in a number of different item samples; in fact, he used a balanced incomplete block design. Actually, Lord has shown that it always pays to administer many different batches of items (in the limit a single item) rather than only one batch, which was the practice in the early days. This is so because the greater number of replications of each item gives better estimates of item difficulties which, when averaged out, lead to more accurate estimates of the population mean (Lord & Novick 74, p. 256).

The possibility of using item sampling to estimate individuals' total test scores was examined by Kleinke (47) with reasonable success. Using a simple regression model, he was able to predict a person's score on the items he did not take from the scores of those who did, and so estimate a total test score. One would not expect item sampling to be efficient for people at the ability extremes. Tailored testing, a limiting form of unmatched item sampling, would be preferable in such cases.

A theoretical problem which has hardly been investigated empirically is that of determining the relative efficiency of different item-person sampling designs when two or more population moments are to be estimated. Apart from statistical considerations, there is the question of information costs, both person-wise (expenditure of time) and item-wise (administration costs). While at first these will generally only come into play in extreme situations where, in any case, the policy will be obvious, as experience with them grows, they may come to exercise greater weight in deciding the best trade-off between persons and items.

Decision-theoretic considerations.—With the publication of the second edition of their book, Cronbach & Gleser (22)⁸ once again challenged test

⁸ Although the book appeared in 1965, it was not reviewed by Keats (44).

theorists to take some notice of decision theory. Their work is frequently cited, but does not yet appear to have influenced applied studies. This may reflect the fact that the mathematical treatment and notation of the book is somewhat demanding of the reader.

The theme of the book is, nevertheless, quite simple; the practical uses to which a test is put and the measurement procedure should not be separated. When decisions are based on test scores, it is the accuracy of classification which matters so that precision of measurement is valuable only insofar as it enhances the quality of the decision, i.e. reduces misclassification. Cronbach & Gleser also emphasize the fidelity-bandwidth dilemma. In essence they see test consumers faced with a choice between a "narrow-band" test, which gives high validity but only with one or perhaps two criteria, with the "broad-band" test, which gives only fair but potentially useful correlations with a number of criteria.

To Cronbach & Glesner also goes the credit for reviving two ideas which Fisher was at pains to stress,⁹ namely, that information must be purchased, and that costs should be assessed as accurately as circumstances permit. If x seconds of computer time are spent obtaining y units of information about a person, then the aim is to achieve the best trade-off between x and y , where some value has been placed on information. Similarly, in the item sampling random effects model, it is of interest to determine the optimum numbers of people and items needed to get "best" estimates of the moments of the population distribution of ability. Another example of cost awareness would be Cleary & Linn's (17) introduction of cost restraints into a discussion of the power of a statistical test.

A rather different treatment of classification problems, couched in the modern test theory idiom, was contributed by Birnbaum (8), who demonstrated, among other things, that only the logistic test model, by virtue of its sufficiency, leads to a uniformly best classification rule. In this context, "best" means that no other rule can have a smaller error probability for one value of θ without some corresponding loss for another value in the appropriate range. With models of essentially different form some loss of efficiency is always involved. In fact, Birnbaum derives expressions for error probabilities (or Type I and II errors) which reveal clearly how, given a particular item characteristic function, the concept of information is instrumental in discriminating between contiguous abilities. His results may be generalized from two to n -point classifications.

Wood (128) has described a Bayes decision theoretic approach to classifying educational achievement. Items are randomly sampled from an ap-

⁹ . . . there is nothing in the nature of scientific work which requires that the allocation of the resources to the ends aimed at should be in any degree rougher, or less scrupulous, than in the case of a commercial business." *Design of Experiments*, p. 184.

propriate population, defined by a subject unit, and administered sequentially to subjects. After each item response, the information accumulated is assessed to see whether a terminal decision (e.g. go to treatment or review this unit) can be made or whether more information must be collected. The best policy at each point is the one which minimizes a suitable loss function, weighted according to prior knowledge about the subject's achievement. The procedure has obvious affinities with tailored testing but without the probabilistic item response model. Items are intended to be formally equivalent, according to item forms theory (see next section). It may be more fruitful, however, to restore the item response model, admit variable item difficulty and discrimination, and integrate the tailored testing procedure with Birnbaum's (8) work described above, while combining the Bayesian approach of Owen (9).

Cronbach & Gleser (22) argued that psychometrician's preoccupation with reducing measurement error in a bid to ape the physical sciences has dominated test theory. Such a view must be respected, and it has some substance, but the fact remains that unless the user knows in advance the ability levels which will define class boundaries, he has no alternative but to attempt to obtain uniformly good precision throughout the range. This justifies the traditional concern with measurement error rather than errors of misclassification.

Item construction theory.—Considering the faith we place in test item responses as valid tokens of behavior, it is surprising how little we know about the psychological properties of items. If we knew what made items work, then we might be able to design them rationally or at least reproduce similar kinds of items at will. Unfortunately, neither capability is securely within our grasp. Test theory has glossed over this disconcerting state of affairs by invoking words such as "equivalence," "homogeneity," "parallel," and "unifactor" to describe item populations which do not actually exist. Obviously what is needed are algorithms of sufficient power to construct such populations.

In the achievement area, Bormuth (14) has devoted a recent book to item generating algorithms. He believes that achievement test items can lead to interpretable results if and only if they can be related directly to instruction and can be derived directly from it by means of a set of operational procedures. The effect of this is to free measurement from the idiosyncracies of the item writer and from introspective judgements about the cognitive processes which items ostensibly elicit.¹⁰ From such an analysis it is possible to enumerate, in a rigorous fashion, the entire population of items

¹⁰ A PhD thesis on this subject is in preparation. (Bortnick, R. *A Response Behavior Model for a Class of Achievement Test Items*, Dep. of Education, University of Chicago.)

relevant to some particular instruction. A random or stratified random item sample—a domain-referenced achievement test (DRAT) in the current jargon—may be drawn as a means of estimating a person's generic true achievement score on the instructional content. A closely allied concept, due to Osburn (88), is the *item form*. An item form is a principle or procedure for generating a subclass of items having a definite syntactical structure. Item forms are composed of constant and variable elements, and as such, define classes of item sentences by specifying the replacement sets for the variable elements. Actual enumeration of items is not necessary although in principle it can be done.

The notion of a universe of items was sharply criticized some years ago by Loevinger (64). She maintained that no such thing could exist since items are not unique as people are—they are not what she calls basic particulars, meaning material bodies—and also that one cannot catalog or display all the members of an item population. At the time she wrote, Loevinger would not have been aware of item forms analysis so that her last criticism is now met. Her first probably stems from a too literal demographic interpretation of the word “population,” and in fact she does entertain the notion of a population defined by a generating rule, as, for example, a population of electric light bulbs.

In fairness to Loevinger, however, it must be said that problems remain in the realization of item forms and associated schemes. Doubts arise because the item construction rules depend largely on linguistic analyses which may not necessarily have psychological or educational salience. At present there are few applied studies to clarify the situation. Guttman & Schlesinger (34) have presented a technique for systematically constructing item responses, and particularly distractors, which bears a close affinity to item forms analysis. Methods of testing the items contrived in this way for homogeneity, or congeniality, via Guttman-Lingoes smallest-space analysis, were reported later (106). Such tests of fit seem necessary since the item form arises from human reasoning and is liable to error. Violations and confirmations of homogeneity ought to lead to improvement of the item generating rule. However, clustering procedures such as smallest-space analysis have been criticized for the absence of an underlying model (Fleiss & Zubin 30), thus reintroducing arbitrariness into the item construction procedure.

Hively, Patterson & Page (38) conducted a study in which they constructed “universe-defined” arithmetic tests. Various hypotheses concerning subject and item class variance components were tested, but within-item class variance did not differ as much from between-item class variance as the theory would have predicted.

Item scoring theory.—The possibility of finding item response weighting schemes which would recover more information about subjects than the ordinary zero-one scheme has always attracted psychometricians [for a re-

view, see Stanley & Wang (113)].¹¹ Recently, interest has centered on schemes which are revealed to the respondent and where he is given the opportunity to maximize his score. One of the more interesting of these schemes is that of Shuford, Albert & Massengill (109) who, following De Finetti's (25) treatment of partial information, ask the subject to attach weights expressing his confidence in the alternative responses to an item under the restriction the weights add to unity. To these weights is applied a scoring system which we leave undefined here. It is assumed that the subject's degree of belief or personal probability concerning the correctness of each alternative answer corresponds exactly with his personal probability distribution, also restricted to sum to unity. If this assumption is correct, the subject has given himself the best opportunity to maximize his expected score on the test. Scoring functions which effect this result are called reproducible scoring systems. Various types of scoring functions have been considered by Shuford et al (109). For the more complicated functions, like the logarithmic, subjects appear to have difficulty understanding the instructions about the pay-off structure and so lose their chance of maximizing their scores (Ebel; 26 but see Winkler 127). On the other hand, if subjects are not briefed about this particular scoring system, its measurement value appears to deteriorate (Rippey 99). Rippey (100) subsequently investigated the reliabilities of tests scored by various functions, under no briefing conditions, and found best results when subjects were assigned a score equal to the sum of the probabilities they had allocated to the correct responses, this being the simplest of all pay-off models. Comparisons of information yields are not available, although Samejima's (105) model for the graded response model may be useful in this connection.

The other "novel" scoring system was proposed by Ramsay (94). The idea is to score items so as to maximally discriminate between members of two known criterion groups. It amounts to establishing a discriminant function where the coefficients are the item weights. The scheme is quite attractive but, as Ramsay himself remarks, relies heavily on the continuing stability of the criterion groups.

The hope has been that continuous response scoring models would curb guessing, the idea being that people will act rationally and honestly where their best interests are involved. But, as Kogan & Wallach (50) have noted, the variation in propensity for risk taking, combined with the variation in the coolness with which people appraise their chances, act to defeat predictions based on strictly rational behavior. Besides, subjects may not care about the alternatives offered them, in which case talk of belief is fatuous. Empirical evidence from other fields suggests that often subjects have a hard time distributing their probabilities; some fail to constrain the probabilities

¹¹ Incidentally, their statement (p. 26) that Samejima's graded response model does not work is simply not true.

to add to one (Peterson & Beach 93), while others tend to lump the probability density on what they consider is the correct answer. Experiments with building up personal probability distributions seem to work better with attributes which are visible and easily understood, e.g. red hair, spectacles (Winkler 125, 126).

These reservations aside, confidence weighting schemes appear to be worth investigating for their information-producing potentialities. Doing such testing via computer should alleviate some of the difficulties hitherto experienced. It should be easier to train subjects in pay-off strategies (they may ask for examples until they are *au fait* with the procedure), and as we speculated earlier, the guessing problem can be tackled in a number of ways, by exploiting the flexibility of the computer. Actually, we don't really know whether people are more or less likely to guess on a computer test than on a pencil-and-paper test. Perhaps the computer could be programmed to establish instrumental control over guessing behavior.

Time-related models.—The suggestion that response latency may be a discriminating variable, with psychometric implications, was revived by Eysenck (27). In a rebuke of psychometrics for neglecting the findings of experimental psychology, he suggested that speed of individual item solution, the nature of errors, persistence before abandoning items, and "other similar differential indication of response style" should be incorporated into any analysis of test performance. He made no attempt to formalize these assertions and avoided the obvious question of how to estimate the extra parameters that would be introduced into the probabilistic response model. Eysenck did, however, cite Furneaux' (32) rather startling finding that the relation between item difficulty and log latency is linear, with the same slope for all subjects. In other words, the only source of difference in intellectual ability is in terms of the intercepts on the log time axis. This result does not seem to have been replicated, but it is clearly of interest that it should be investigated in general settings. In some respects it seems contrainuitive, since one would expect low scorers to abandon hard times quickly.

Many psychometricians may still be unfamiliar with the reformulation of the classical test theory model, due to Woodbury (129), and elaborated by Novick (80), which characterizes a test performance as the realization of a time-dependent stochastic process. A complete statement of the theory may be found in Lord & Novick (74, Ch. 5). Basically, the length of a test is associated with a discrete time-parameter t , and the observed score and error score random variables are regarded as functions of time, where the score may be number of words a person reads correctly in t minutes or perhaps the time he takes to read t words correctly. The model offers a satisfying formulation of specific true score as the observed score on a test of infinite length, and this leads to an interesting interpretation of residual covariance as the length of the time interval over which the two residuals overlap.

Woodbury's model provides a conceptual basis for adjusting relative test lengths to maximize the multiple correlation of a test battery for a fixed testing time. This is the problem Lord & Novick (74, p. 124) regard as "the most complex, and in many respects, the most interesting problem in classical test theory." Weights and administration times may be assigned to tests so as to maximize either the validity or the reliability of the composite. Thus the problem has a distinct decision-theoretic character and has now been intensively studied (42, 130).

In the classical test theory model, true score is assumed to remain fixed over replication of the same items. The same applies to the latent ability in a probabilistic response model. Vinsonhaler & Meredith (122) considered the situation where the latent random variable undergoes a systematic change after each presentation of the same item. They conceived of multiple presentations of the same item as a stochastic process, i.e. a series of inter-related stochastic events, and discussed appropriate stochastic models for representing this state of affairs. Latent change theory, as the authors call it, has implications for the study of test reliability where it is suspected that systematic errors are accumulating over a series of test repetitions. Applied studies based on this approach may be rewarding.

Although speeded tests are widely used in aptitude assessment, psychometric theory had had relatively little to say about them. An exception is Meredith (78), who explores the consequences of assuming that the distribution of the subject's score over successive intervals is Poisson, as well as the implications of assuming that the latent process generating the score for a given individual is Poisson. Meredith acknowledges Rasch's (95) prior work in this field, but does not adopt Rasch's multiplicative parameter structure.

The most recent work in this area is that of Owen (90), who has done a Bayesian analysis of Rasch's model for misreadings. The analysis turns out to be especially fruitful in that the results have a direct application to estimating Rasch's more general logistic model. Joint modal estimates and posterior distributions of person and item parameters are derived for the cases where (a) both person and item parameters are unknown, and (b) either the person or item parameters are known. Both correspond to important practical testing problems—item calibration and up-dating, and repeated measures on the same people. Owen (90) also provides the necessary results for parameter estimation with unmatched item sampling designs, although only for the fixed case. This treatment is made more explicit in Novick (82). Owen also, of course, paves the way for Bayesian sequential item testing, which has always been immanent in the Rasch model.

7. COMPUTER PROCEDURES

"Bread and butter" item and test analysis programs which merely tally responses, give proportions correct, compute part-whole correlations, and print test scores are not included here because they are too numerous and too

undistinguished. (The reader is referred to *Educational and Psychological Measurement* and *Behavioral Science* as a source of these programs.)

A flexible general purpose item analysis package called FORTAP has been written by Baker & Martin (7). FORTAP supplies estimates of the item parameters for the distractors as well as the correct response—an unusual feature and one that proves to be useful for diagnostic purposes. It also provides a welter of more commonplace item and test statistics, including reliabilities based on Hoyt's original analysis of variance method.

The procedure for estimating item and ability parameters, assuming the normal ogive model and dichotomous item scoring, has been programmed in FORTRAN IV for the IBM 360/65 by Kolakowski & Bock (52). A program for the nominal response model has been written by the same people (53). Simultaneous estimation of item and ability parameters, assuming the Rasch model, can be effected using a program written by Wright & Panchapakesan (133) in FORTRAN IV for the IBM 360/65.

Miscellaneous programs of interest include a program to score items by various scoring functions (Rippey 98) and a program which solves an important problem discussed by Cronbach & Gleser (22), that of selecting a subset of two predictors from a set of predictors to be used in the two-stage selection procedure and of determining the appropriate cutting scores on the first stage test (Rock & Barone 101). A program for optimally allocating testing time (Thayer & Novick 116; see also 85), written in FORTRAN IV for the 360/65, is available. Wingersky, Lees, Lennon & Lord (124) report a program for estimating true score via empirical Bayes methods. A program which facilitates the construction of universes of items and subsequent random sampling has been reported by Shoemaker & Osburn (108).

8. CONCLUSION

It seems appropriate to conclude a review devoted to progress in test methodology with some speculation about the future of these methods in applied settings. Although many of the papers reviewed here substantiate Anastasi's (2) observation that psychometricians "appear to shed much of their psychological knowledge as they concentrate on the minutiae of elegant statistical techniques," they rarely contain any novel mathematical or statistical results and must be justified by their implications for test practice. One cannot expect, however, that all users of psychological tests are equally ready to receive these methodological innovations.

In particular, we find it hard to be sanguine about the prospect for any immediate effect on that largest and most visible user of tests, the educational enterprise. The problem in educational practice is that tests are not actually used in a critical way, i.e. they serve only to rank students for purposes of administrative decisions. Since the test is competing only with the alternative of a subjective or random decision rule, it matters little that the ranking provided by the test may be only approximately correct. In this

setting, the user has no direct evidence of the good or poor measurement properties of the test. He can only rely on the test publisher's claims for reliability and validity. Faced with this uncritical use of his test, the publisher in turn has little incentive to investigate thoroughly the operating characteristics of the test, let alone to improve them.

Regrettably, the situation is not much better in educational evaluation and research. The focus in these fields is on group statistics—means, variances, correlations, etc.—which are robust in the presence of measurement error and unfavorable score distributions. Thus, the labor involved in psychometric refinements of the test is not repaid by any dramatic improvement in research outcomes. Again the incentive for innovation in test methods is lacking.

The new developments will more likely have a prompt impact in areas of research where problems of measurement error and the metric properties of test scores are presently hindering progress. One of these areas is the study of individual growth curves for behavioral traits, such as abilities and attainments, which are operationally defined by means of tests. It is well known that a high degree of measurement precision is required to reveal the fine structure (i.e. short-term changes) in individual growth. Even certain physical measurements, e.g. trunk circumference, are insufficiently precise to demonstrate stable yearly changes. Typical standard and commercial tests designed to characterize differences between individuals rather than changes within individuals probably are not accurate enough for this type of application. Psychologists interested in individual growth in behavioral traits are not likely to make headway until they press into service every possible device, psychometric and otherwise, to increase the information content of their tests. This advice applies especially to the cognitive psychologists who, having had little exposure to the psychometric discipline, often attempt to characterize cognitive development with instruments which are woefully inadequate to the task.

Another area of research which is now expanding rapidly, and where measurement problems are critical, is human behavior genetics. In genetic analysis, one is often critically interested in differences between family members. Since many traits show considerable intraclass correlation with respect to classification by family, the problem of accurately assessing such differences is much more difficult than that of assessing differences between unrelated individuals. Furthermore, recent interest in the detection of major genes underlying quantitative traits raises the question of whether the population distribution of the trait shows evidence of being a mixture of normally distributed components. This question can be investigated only when the scale of measurement has a well-defined metric. In order to obtain such metric scores from a psychological test, a strong model for the score or the item response is required. Because of the recent advances in modern test theory, the psychometric basis for effective research in behavior genetics

appears to be at hand. Interdisciplinary work between these fields presents a bright prospect for elucidating the genetic bases of many aspects of behavior.

A final area of research which is presently receptive to new test methodology is personality measurement. Here the problem is not so much one of precision and metric as one of developing response models which are qualitatively more complex than the simple one-dimensional models of ability testing. Models for personality measurement will have to encompass effects of response bias as well as possibly more than one dimension of content. Preferably, these models should describe responses at the item level rather than the factor-analysis type of models for scores on multi-item scales. The use of a latent trait model for this purpose has already been attempted by Damarin (24). Further work along these lines may be rewarding.

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STATISTICAL THEORY **(Behavioral Science Application)**

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INTRODUCTION

In this review, an attempt has been made to examine critically recent developments in areas of statistical theory of particular relevance to behavioral science. Three areas are of special importance: simultaneous inference, multivariate analysis, and nonparametric theory, and these are examined intensively. The years 1967 and 1968 are covered, but a number of important papers have appeared before and after this period and these are included.

SIMULTANEOUS INFERENCE

The most important development in this area has been the appearance of the book by Miller (45). At last there is available in one source all the important multiple comparisons procedures, both parametric and nonparametric. The accompanying critical discussion of the relative merits of the various procedures is particularly valuable. So too is the discussion in the introduction (pp. 31-35) of the relative merits of per-comparison versus per-experiment significance levels. Miller feels this must be an individual decision:

... The antithesis of the ultraconservative statistician is the nonmultiple comparisonist, who only controls the statement error rate. His contention is that if a statistician is willing to tolerate an error rate of α for separate, independent statements, there is no reason to change one's philosophy for a sequence of dependent (or independent) statements just because some of them happen to be related ...

Provided the nonsimultaneous statistician and his client are well aware of their error rates for groups of statements, and feel the group rates are either satisfactory or unimportant, the author can find no quarrel with them. Every man should get to pick his own error rates. Simultaneous techniques certainly do not apply, or should not be applied, to every problem. However, there are numerous problems in which the context of the problem calls for greater protection of the joint null hypothesis ...

... For illustration, consider a two-way classification (fixed effects) analysis-

¹ I have benefited from helpful discussions with R. J. McKay and A. J. Sutton.

of-variance problem with multiple observations per cell. If the number of rows and columns is not excessive, and the degrees of freedom are adequate, the author might like to treat all statements on row effects, column effects, and interaction effects as one family. For a large two-way design, however, this may not be practical. A less conservative grouping would be the row effects statements as one family, column effects as another, and interactions another. . . . It is unlikely that the author would subdivide further and treat each row statement separately in a nonsimultaneous fashion unless there were just a few specific comparisons spotlighted before the start of the experiment. *There are no hard-and-fast rules for where the family lines should be drawn, and the statistician must rely on his own judgment for the problem at hand.* (Miller's emphasis.)

It seems appropriate to summarize here the relative advantages of the competing multiple comparisons methods in the simple one-way classification. The main competitors are Tukey's studentized range (T) method and Sheffé's S method. These are both all contrast procedures, i.e. when using either method at level α , simultaneous tests on all contrasts of means with per experiment (family) error rate α may be carried out. For contrasts involving only pairs of means, the T method is superior; for more general contrasts the S method is superior. In practice the experimenter is interested in only a finite number of contrasts, and in this case a competitor of the S and T methods is the Bonferroni t method (Bt), which should be much better known than it is. If exactly r contrasts are to be tested, a family error rate of at most α is obtained if the r contrasts are tested using Student's t at level $\alpha/2r$. This method can often improve considerably on the others when the number of contrasts r is not much larger than the number of groups, for the critical value is directly related to the number of contrasts to be tested. For example, in a three-group problem with equal sample sizes and error degrees of freedom $\nu=30$, suppose that only the three pairs of mean differences are to be tested. The critical values at the 5 percent level for the studentized contrasts are then $q_{3,30}^{.05}/2^{1/2}=2.47$ for the T method, $\{2F_{2,30}^{.05}\}^{1/2}=2.58$ for the S method, and $t_{30}^{.025/3}=2.36$ for the Bt method. It is then perfectly legitimate to use the smallest critical value for the three comparisons of means, since this decision depends not on the data, but on the power properties of the competing methods. Tables of the studentized range and Bonferroni t are given in Miller. Unfortunately, the former is restricted to $\alpha=.01$ and $.05$ and the latter to $\alpha=.05$. The latter may be calculated for any α from tables of the incomplete beta function.

The two other methods in common use are the Newman-Keuls (NK) and Duncan (D) multiple range tests. The D method is regarded with suspicion by some statisticians [see for example Scheffé (59, p. 78), Gabriel (17, p. 1095), and Miller (45, pp. 81-89)]. In the above three-group problem, suppose that the sample means are $m_1 < m_2 < m_3$. The D method first compares the studentized range $(m_3 - m_1)/\hat{\sigma}(2/11)^{1/2}$ with $q_{3,30}^{1-(0.95)^2}/2^{1/2}=2.15$, and if this is significant, the two remaining comparisons of means are compared with $q_{2,30}^{.05}/2^{1/2}=t_{30}^{.025}=2.04$. The first stage of the NK method is

the same as that of the D method, but uses the critical value $q_{3,30}^{.05}/2^{1/2} = 2.47$ as for the T method. The second stage is the same as that of the D method.

Miller favors the NK method as the best in the one-way classification, for while it maintains the same α level protection for the null hypothesis as does the T method, it also provides α level tests for subhypotheses explicitly tested, whereas the significance level of the T method for subhypotheses may be much less than α . It is clear that the NK and D methods are not all contrast procedures in the sense of the S and T methods; in fact, as Miller notes, the two methods cannot be used for constructing simultaneous confidence intervals for the population mean differences. This is surprising, in view of the general relationship in statistical theory between hypothesis testing and confidence interval estimation, but the reason becomes clear if one examines the logic of the tests carefully. The first stage of the NK method is a range test of the hypothesis $\mu_1 = \mu_2 = \mu_3$ at level α . If this hypothesis is rejected, the two hypotheses $\mu_1 = \mu_2$ and $\mu_2 = \mu_3$ are then tested at the same level α , but using a two-mean range test (i.e. t test) rather than the three-mean range test used at the first stage. The equivalent procedures in terms of confidence intervals are that at the first stage simultaneous confidence intervals of the form

$$\mu_i - \mu_j \in m_i - m_j \pm \hat{\sigma}q_{3,30}^{.05}/(11)^{1/2}, i, j = 1, 2, 3, i \neq j$$

are provided for all three pairs of mean differences. If any of these intervals does not contain the value zero, the second stage is performed, and this gives confidence intervals

$$\mu_1 - \mu_2 \in m_1 - m_2 \pm \hat{\sigma}q_{2,30}^{.05}/(11)^{1/2}$$

$$\mu_2 - \mu_3 \in m_2 - m_3 \pm \hat{\sigma}q_{2,30}^{.05}/(11)^{1/2}$$

for the mean differences $\mu_1 - \mu_2$ and $\mu_2 - \mu_3$. Thus we have generated two different confidence intervals for these two mean differences if the null hypothesis is rejected, so that the length of the confidence intervals for $\mu_1 - \mu_2$ and $\mu_2 - \mu_3$ depends on that for $\mu_1 - \mu_3$. This logical difficulty is not removed by restricting the NK method to testing rather than estimation.

This difficulty has been pointed out by Gabriel (16), who advocates the use of simultaneous test procedures (STPs) in this and other situations (16-20). An STP in this context is a procedure for testing the homogeneity of all subsets of means of any size with a prescribed per experiment error rate α . Tukey's T method is an STP, and Gabriel shows that Scheffé's S method may be adapted to provide tests on all subsets of means, by comparing the among group sum of squares for any subset of means with the within-group mean square based on all groups, using the critical value for the overall F test. The particular feature of the STP is that all tests of significance are

carried out against the same critical value, with no reduction of the critical value for testing subhypotheses involving smaller numbers of means. The STP is also *transitive*, in the sense that if a particular set of means is declared significantly nonhomogeneous, then any larger set containing this set will be declared significantly nonhomogeneous, while if the set is not significantly nonhomogeneous, no subset of it can be declared significantly nonhomogeneous. The *NK* method does not possess this property, and it is quite possible for two out of a set of five means to be declared significantly different by the *NK* method at the 1 percent level when the entire set of five is just significantly nonhomogeneous at the 5 percent level. It is also possible for the size of some tests in the *NK* method to rise above the nominal level, as shown in a sampling study by Petrinovich & Hardyck (49).

It is clear that the *D* method is not directly comparable with the others, as its per-experiment error rate is not .05 but $1 - (0.95)^2 = 0.0975$ (in general, $1 - (1 - \alpha)^{k-1}$ where k is the number of groups), though it can, of course, be made comparable by using $\alpha' = 1 - (1 - 0.05)^{1/2} = 0.0253$. The tables of the *D* method give $\alpha = .05$, not $\alpha' = .05$, however, and it is therefore unreasonable to carry out an *F* test at the 5 percent level and then follow this with a *D* method using $\alpha = .05$, as is sometimes advocated. Senter (62, pp. 283-91), in fact, does the *F* test at the 1 percent level and the *D* method at the 5 percent level, making the imbalance worse. In fact, the intention of the *D* method is to test individual pairs of means at the per-comparison 5 percent level, and the level chosen for the overall range test, $1 - (1 - 0.05)^2$, is meant to correspond to the per-experiment level obtained from two independent tests each at the 5 percent level. This procedure is discussed at some length by Miller (pp. 81-89).

Rodger (54, 55) has carried Duncan's argument to its logical conclusion in developing a modification of the *S* method to provide a per-contrast error rate. He claims, in effect, that an overall *F* test of homogeneity in a k group problem will, if carried out at level $\gamma' = 1 - (1 - \alpha)^{k-1}$, give α level t tests on all contrasts. It is well known that an α level *F* test corresponds to testing all contrasts by the *S* method using the critical value $\{(k-1)F_{k-1, \nu}^{\alpha}\}^{1/2}$, so Rodger's claim is that

$$\{(k-1)F_{k-1, \nu}^{1-(1-\alpha)^{k-1}}\}^{1/2} = t_{\nu}^{\alpha/2}$$

It can be verified from tables that this is incorrect. A more detailed criticism is given by Aitkin (1), in which the correct relation between the per-experiment level γ and the per-contrast level α is given as

$$\gamma = P\{F_{k-1, \nu} > F_{1, \nu}^{\alpha}/(k-1)\}$$

or

$$\alpha = P\{F_{1, \nu} > (k-1)F_{k-1, \nu}^{\gamma}\}$$

This relation has also been given essentially by Gabriel (16) and others.

Tables of the incomplete beta function are required in general to evaluate the probabilities α and γ .

It is worth noting that an orthogonal contrasts procedure can be modified to give a specified per-experiment error rate. The appropriate critical value is that of the studentized maximum modulus distribution, as described (and tabulated for the .05 level) in Miller. This will be more powerful than the *Bi* method proposed by Hays (29, p. 489).

Miller's book deals comprehensively with nonparametric multiple comparisons. In general, methods based on rank tests are more useful than those based on sign tests. Rank analogs of the *S* and *T* methods are the Kruskal-Wallis (*KW*) rank method and the Steel-Dwass (*SD*) rank method. The former is based on the usual Kruskal-Wallis statistic, the latter uses the maximum Wilcoxon two-sample rank statistic over all possible pairs of samples. The *KW* method requires only one ranking of all the observations in all the samples, but the test statistic for the significance of the difference between mean ranks in any two samples depends implicitly on the observations in all the other samples. This method also cannot provide simultaneous confidence intervals for differences in location parameters. The *SD* method requires the separate ranking of every pair of samples, but the test statistic for any two samples consequently does not depend on the observations in the other samples. This method does provide simultaneous confidence intervals for all location differences.

Multiple comparisons in contingency tables are dealt with only briefly by Miller, in summarizing work done by Goodman (23, 24). Since these are rarely employed in practice, an outline of the available techniques seems useful. Consider r populations in each of which an observation may fall into any of c categories. A sample of n_i is drawn from the i th population, and n_{ij} of these fall in the j th category, giving an $r \times c$ contingency table. The null hypothesis of homogeneity $p_{ij} = p_i$, $i = 1, \dots, r$, $j = 1, \dots, c$ is rejected at level α if

$$X^2 = \sum_i \sum_j (n_{ij} - n_i \hat{p}_j)^2 / n_i \hat{p}_j > \chi_{(r-1)(c-1)}^{2\alpha}$$

where

$$\begin{aligned}\hat{p}_j &= \sum_i n_i \hat{p}_{ij} / n \\ \hat{p}_{ij} &= n_{ij} / n_i \\ n &= \sum_i n_i\end{aligned}$$

Simultaneous confidence intervals at (per-experiment) level α for all contrasts

$$\sum_i \sum_j c_{ij} \hat{p}_{ij} \quad \left(\text{with } \sum_i c_{ij} = 0, j = 1, \dots, c \right)$$

are given by

$$\sum_i \sum_j c_{ij} \hat{p}_{ij} \in \sum_i \sum_j c_{ij} \hat{p}_{ij} \pm [\chi^2_{(r-1)(c-1)}]^{1/2} s_c$$

where

$$s_c^2 = \sum_i \left[\sum_j c_{ij}^2 \hat{p}_{ij} - \sum_j (c_{ij} \hat{p}_{ij})^2 \right] / n_i$$

If only a small number m of contrasts is of interest, the critical value $\lambda^{\alpha/2m}$ may be substituted for $[\chi^2_{(r-1)(c-1)}]^{1/2}$ to give shorter intervals as in the Bt -method, where $\lambda^{\alpha/2m}$ is the upper 100/2m percent point of the normal distribution.

Consider an example given by Edwards (14, p. 375) of a 3×3 table in which samples from three educational level populations are classified according to their response to an item in an opinion poll.

	AGREE	UNDECIDED	DISAGREE	TOTAL
College graduate	10 (.105)	35 (.368)	50 (.526)	95
High School graduate	20 (.286)	40 (.571)	10 (.143)	70
Elementary School graduate	35 (.412)	40 (.471)	10 (.118)	85
Total	65	115	70	250

The $\hat{p}_{ij}$ are given in brackets after the n_{ij} . The overall test of homogeneity gives an X^2 of 53.38, significant beyond the 0.1 percent level of χ_4^2 . Suppose we want simultaneous confidence intervals at the 5 percent level for the differences

$$\hat{p}_{11} - \hat{p}_{21}, \quad \hat{p}_{11} - \hat{p}_{31}, \quad \hat{p}_{21} - \hat{p}_{31}$$

between probabilities of agreement in the three populations, and

$$\hat{p}_{13} - \hat{p}_{23}, \quad \hat{p}_{13} - \hat{p}_{33}, \quad \hat{p}_{23} - \hat{p}_{33}$$

between probabilities of disagreement. For the difference $\hat{p}_{11} - \hat{p}_{21}$ we have

$$\begin{aligned} s_c^2 &= (\hat{p}_{11} - \hat{p}_{11}^2)/95 + (\hat{p}_{21} - \hat{p}_{21}^2)/70 \\ &= 0.003, 906, 411 \\ s_c &= 0.0625 \end{aligned}$$

which is just the standard deviation of the difference between two binomial proportions. The critical value for all contrasts is $(\chi_4^{2.05})^{1/2} = 3.081$. Since we wish to test only $m=6$ contrasts, the smaller critical value $\lambda^{.025/6} = 2.638$ would be preferred. The confidence interval for $\hat{p}_{11} - \hat{p}_{21}$ is then

$$\hat{p}_{11} - \hat{p}_{21} \in (.105 - .286) \pm 2.638 \times .0625$$

or

$$-0.346 < p_{11} - p_{21} < -0.016$$

and similarly for the others (S_G will of course be different for each contrast). It is worth noticing that the test of the hypothesis $p_{11}=p_{21}$ implicit in the above confidence interval corresponds to an ordinary X^2 test for independence in the 2×2 contingency table

10	85	95
20	50	70
30	135	165

except that the value of X^2 is compared, not with $\chi_1^{2.05}$, but with the smaller of $\chi_4^{2.05}$ and $(\lambda^{.025/6})^2$. Thus the value of X^2 is 8.822 which exceeds $(2.638)^2=6.959$, leading to rejection of the hypothesis $p_{11}=p_{21}$. Equivalently, the confidence interval does not contain zero.

This procedure for testing subtables of the full $r \times c$ table should be distinguished from the partition of χ^2 due to Lancaster in Kendall & Stuart (38, p. 577) which is essentially a partition into $(r-1)(c-1)$ orthogonal components. As for orthogonal contrasts in ANOVA, the latter partition must be decided before examination of the data, which limits its usefulness.

Gabriel (17) has developed an STP for the contingency table problem, to test the homogeneity of all subtables of the main table. He points out that the Pearson X^2 used above is not a monotone statistic, i.e. its value is not necessarily greatest for the complete table (in contrast to the among-group sum of squares or range in ANOVA, which are nondecreasing functions of the number of groups). This is awkward, as it could lead to the rejection of the homogeneity hypothesis for a subtable, but not for the main table. To avoid this problem, Gabriel uses the information or likelihood ratio statistic for the homogeneity hypothesis, which is monotone, and has the same asymptotic χ^2 distribution as X^2 (to which it is asymptotically equivalent).

MULTIPLE REGRESSION

A useful book in this area is that by Draper & Smith (13). The book provides a standard course in multiple linear regression, using summation notation initially but quickly changing over to matrix notation. Proofs are largely dispensed with, but good chapters are provided on the examination of residuals, the choice among regression equations, and nonlinear regression. The chapter on selecting the "best" regression equation illustrates the fundamental difficulty in which users of multiple regression find themselves: how does one decide which variables may be eliminated from the set of predictor variables as irrelevant? As shown in this chapter, the forward, backward, and stepwise methods of selecting variables may lead to different conclusions as to which equation is the "best."

In one sense, of course, there is only one "best" equation, namely that

using all the predictor variables, for this will have the highest R^2 . However, if any of the population regression coefficients is zero, the corresponding variables may be omitted with no reduction in the population multiple correlations. Further, Walls & Weeks (67) point out that the addition of a variable to a regression equation can never reduce, and usually increases, the variance of a predicted response, so that it is desirable for accuracy of prediction to reduce the number of predictor variables.

The elimination (or addition) of variables is usually performed using a variation of the t test for a regression coefficient. In its simplest form this amounts to omitting variables if their t values do not exceed the critical value $t_{p, \alpha/2}$ for some α . More sophisticated procedures are the above-mentioned forward, backward, and stepwise methods. All these methods use variations on the t test. Geary & Leser (21) point out some apparent paradoxes in procedures of this kind. It occasionally happens, for example, that R^2 is significant by an α level F test, but none of the individual t tests is significant at level α . Geary & Leser examine conditions under which this may occur.

Inconsistencies of this kind have led to attempts to redefine the meaning of the "best" equation. Gorman & Toman (25), following a method proposed by Mallows, compare regression equations using a simple function of the residual sum of squares. Let E_β denote the residual sum of squares using all p predictor variables, and E_γ the residual sum of squares using a subset of $p-r$ of the p variables. Define

$$C_\gamma = (n - p - 1)E_\gamma/E_\beta - \{n - 2(p - r + 1)\}$$

where n is the sample size. Then C_γ is an estimate of the total squared error (squared bias plus random error) of the equation in which the $p-r$ variables are retained. Gorman & Toman suggest that for the purpose of prediction the equation with the smallest C_γ would generally be the best choice. This requires the generation of all $2^p - 1$ regression equations, a formidable task if p is large. Efficient methods for generating all equations are discussed by Hocking & Leslie (30) and Schatzoff, Tsao & Fienberg (58). Beale, Kendall & Mann (7) give a computer algorithm for finding the subset of variables of a given size with the highest R^2 , for all sizes.

These attempts to avoid the significance test problem do not solve the basic question. Given two equations with two different R^2 s (or C_γ s), how do we decide whether one equation is *significantly* better than the other? A new attempt on this problem has been made by Spjøtvoll (66), who points out that the usual stepdown (t -) tests for the significance of additional variables, employed in the various selection procedures, assume that the order of addition or elimination of variables is fixed in advance. This is almost never true, however (polynomial regression is perhaps an exception), as the variables first included (omitted) are generally those with the largest (smallest) stepdown F statistics. Thus the theoretical basis of the usual stepwise procedures is highly suspect.

Spjøtvoll constructs simultaneous confidence intervals for all quadratic

functions of the regression vector β , and uses these to test the difference between the residual sums of squares based on any two subsets of variables. Heavy computations are required for the test except in the special case where one subset of variables is the complete set.

MULTIVARIATE ANALYSIS

Four books published in this area are of special interest. The book edited by Whitla (69) has an excellent 40-page chapter by Bock & Haggard on the use of multivariate analysis of variance in behavioral research. The technique is illustrated by the analysis of an unbalanced two-way classification, the responses being scores on achievement tests in reading, arithmetic, spelling, and language skills. Examples are also given of the use of canonical regression analysis, multivariate analysis of covariance, trend analysis, and analysis of multinomial data.

The book edited by Krishnaiah (40) contains the proceedings of the first International Symposium on Multivariate Analysis held in 1965. Papers were presented in eight sections, of which one was Applications. The theoretical papers illustrate the truly formidable complexity of many of the distributional problems still unsolved in multivariate analysis. The direction of research in multivariate analysis, as indicated in this book and in the journals, has been criticized by a reviewer (10), who refers to the

... almost unbridged gap between the relevant problems in the analysis of multivariate data and the theoretical results presented in the journals.

... Six of the papers in this volume relate to multivariate analysis of variance. . . . Except for one paper solely concerned with design, all these assume that the purpose of multivariate analysis of variance is to test a hypothesis of no difference among groups. In fact, one wants instead information about the *nature* of the differences. . . . The eigenvalues and eigenvectors of the between-group covariance matrix with respect to the within-group covariance matrix can provide such information. This approach is completely ignored in the present papers, and in most published work on this problem. . . .

In a paper in the Applications section, Kempthorne (37) takes an extreme position in claiming that multivariate analysis (excluding principal component analysis) is of little use; since response variables are measured for their intrinsic importance, one wants a univariate analysis on each of the responses, and a multivariate analysis can provide little further information.

The third book, by Morrison (46), develops the Roy largest-root approach to hypothesis testing and multivariate multiple comparisons. The theory of these methods is not dealt with rigorously, but even so some unnecessary complexities in notation and approach make the book difficult in parts. The book is valuable, however, in providing a readable introduction to Roy's union intersection method in multivariate analysis [almost unreadable in Roy (57)] and for numerous real examples.

The fourth book, by Dempster (12), is a disappointment from a user's point of view. The cover flap states " . . . a clear understanding of the geo-

metrical and computational aspects of multivariate methods of data analysis is much more fundamental to a working statistician than are elegantly derived formulas for high dimensional probability density functions. The orientation of the book, therefore, is toward data analysis rather than mathematical statistics." The research worker hoping for a readable account of multivariate methods will not find it. The approach to the subject followed by the author emphasizes coordinate free vector space concepts, which, while giving great generality, ensures that only some professional mathematical statisticians will be able to follow it. Readers who persist, or skip through to the examples of data analysis, will find some techniques advocated which seem to me of dubious value. An example occurs on pp. 172-175, in which the author carried out what is essentially a Hotelling's T^2 test on two groups of student nurses, one successfully completing the first year of training and the other failing to complete the year. The measured variables were 15 from the Edwards Personal Preference Schedule and 16 Cattell personality variables. The T^2 test was performed by defining a dummy variable representing group membership and carrying out a multiple regression of the dummy variable on the 31 predictor variables. In fact, the actual multiple correlation coefficient was never calculated, and instead principal component analyses were performed on the two sets of 15 and 16 variables, the resulting components then being correlated with the dummy variable. Dempster then claimed that only the first four components were needed from each set, as it was only these components which were "meaningfully" correlated with the dummy variable. The correlations were, from the first to the fourth components, -0.155 , 0.005 , 0.076 , 0.129 for the Edwards components, and -0.171 , -0.091 , -0.056 , -0.136 for the Cattell components.

This procedure has serious implications, and is worth discussing at length, as it seems to be advocated quite frequently, as in Darlington (11), for example. First, it is clear that a multiple regression carried out using only the four leading components in each set must give a smaller R^2 than that using all the components (i.e. all 31 original variables), since the weights given to each variable will not correspond in general to the regression coefficients. It is not clear how much reduction there will be, but it may be quite substantial. Second, the absolute values of the correlations of the components do not decrease monotonically, as is seen in the above case. Indeed, there is no reason why they should, as the components are chosen to maximize internal variation of the sets of predictor variables, and ignore the correlations of predictors with criterion. Thus there is no guarantee that all "meaningful" components have been included at any stage, unless *all* are included. Third, in many regression studies the predictor variables will have well defined physical meanings, and it will be neither necessary nor desirable to carry out a principal component analysis. Other comments on this procedure are given by Darlington (11, p. 179).

A second example of canonical correlation analysis (11, pp. 181-86) illustrates this more forcefully. Two sets of measurements were made on 221

nurses, consisting of the above 16 Cattell variables and a set of 31 devised by the experimenter (Smith). Of the 16 canonical correlations, the two largest (0.7515 and 0.6525) are significant at the 5 percent level by a Roy largest root test. Dempster correlates the two sets of principal variables from separate component analyses, and claims that the first five variables in each set contain all the essential correlation between the two sets of variables. The same difficulties are encountered as before, however. There is no monotone decrease in the size of the correlations; for example, the correlations of the first Cattell principal variable with the first, . . . , fifth Smith principal variables are -0.227 , -0.175 , -0.041 , 0.017 , 0.138 , and of the fifth Cattell variable with the Smith variables are 0.005 , 0.029 , -0.079 , 0.360 , 0.128 . Dempster notes that there are some high correlations among the later variables, too (0.243 between the first Smith and the fifteenth Cattell variables, 0.227 between the twenty-second Smith and the eleventh Cattell variables, and 0.205 between the thirty-first Smith and the fifth Cattell variables), but says (p. 185) "it is difficult to believe that these three correlations mean anything more specific than that some small residual tendency for correlation remains after the 25 more promising elements have been removed." The point is that there is no a priori reason for believing that *any* principal variable of one set will be highly correlated with any principal variables of the other set.

The question of significance tests for these procedures seems just as dubious. In the 31 variable regression example above, with a total sample size $n = 264$, a multiple correlation R is declared significant at the 5 percent level if $232R^2/31(1-R^2) > F_{31, 232}^{.05}$, i.e. if $R > 0.4084$. Dempster claims that the distribution theory for principal variables will be the same as that for the original variables, since the principal variables are chosen to maximize internal variation, without regard for correlation with the criterion variable. From this point of view, the significance of a single (say the first) principal variable is assessed in the same way as a single original variable, by its simple correlation with the criterion variable. Thus the correlation r_1 between the first principal variable and the criterion would be declared significant at the 5 percent level if $262r_1^2/(1-r_1^2) > F_{1, 262}^{.05}$, i.e. $|r_1| > 0.1208$. It is clear that the principal component method will frequently find significant associations when multiple regression does not. This does not seem to accord with the Scheffé simultaneous approach to multiple regression, in which the overall F test for the significance of R^2 also provides simultaneous tests for the significance of any linear function of the predictor variables, whether determined in advance or by the data, and hence for principal variables in particular. Indeed, Dempster seems to have the best of both worlds, in that he claims that principal variables will also be good predictors, but the distribution theory advanced for these good predictors is the same as that for individual variables.

At this point, it is appropriate to consider Kempthorne's criticism of multivariate analysis. Just what does a Hotelling's T^2 test at level α tell us

that a battery of p separate t tests at level α does not? The control of the per-experiment error rate obtained by Hotelling's T^2 can also be achieved (conservatively) by a battery of (Bonferroni) t tests at level α/p , so this is not relevant. The important point is that the relative importance of each variable may be quite different when all are considered jointly than when each is considered marginally. This phenomenon is quite familiar in multiple regression (which is often considered not to be a multivariate technique), where some predictor variables which are uncorrelated with the criterion may appear with large regression coefficients, while some with high correlations may appear with zero regression coefficients. The same results hold for Hotelling's T^2 , with correlations replaced by standardized mean differences and regression coefficients replaced by discriminant function coefficients, and for multivariate methods generally. This incidentally shows the undesirability of pre-selecting predictor variables on the basis of high correlations with the criterion *before* a multiple regression is performed, although this procedure is still sometimes advocated.

The use of multivariate analysis of variance (MANOVA) has been somewhat complicated by the fact that there is no uniformly most powerful invariant test for the hypothesis of no group differences on all variables. All invariant tests (under linear transformations of the variables) are based on the characteristic roots of AW^{-1} , where A is the among group matrix of sums of squares and cross products, and W is the within group matrix. If the number of groups is k , and the number of variables is p , the nonzero ordered roots of AW^{-1} may be denoted by $\Theta_1 > \Theta_2 > \dots > \Theta_s$ [$s = \min(k-1, p)$]. At least four test statistics have been proposed for the null hypothesis. The four in common use are:

1. Roy's largest root statistic: Θ_1
2. Wilks' likelihood ratio statistic:

$$W = \prod_{i=1}^s \frac{1}{1 + \Theta_i} = \frac{|W|}{|A + W|}$$

3. Hotelling's generalized T_o^2 statistic:

$$T_o^2 = \sum_{i=1}^s \Theta_i = \text{tr} AW^{-1}$$

4. Pillai's criterion:

$$V = \sum_{i=1}^s \frac{\Theta_i}{1 + \Theta_i} = \text{tr} A(A + W)^{-1}$$

Power comparisons of these four tests have been given by Pillai & Jayachandran (50) for the bivariate case with $k=4, 6, 8, 14$ and various sample sizes. For small departures from the null hypothesis, the differences in power among the tests are small. For larger departures, the power of the

Θ_1 test is below those of the other tests except when there is only one nonzero population noncentrality parameter (i.e. when the k means are collinear in p space). This clearly occurs because the other sample roots also become inflated, and these are ignored by the Θ_1 test. This would seem to weigh against the use of the Θ_1 test, but Gabriel (18) finds the Θ_1 test optimal in a different sense. Just as in ANOVA the F test also provides simultaneous tests on all contrasts of group means, so in MANOVA all four tests provide simultaneous tests for all contrasts of means, on all linear functions of variables. Gabriel shows that, if all four tests have the same size α for the overall null hypothesis, the Θ_1 test has the largest size for tests of pairs of means on individual variables, i.e. there is less reduction in size (and hence power) below α for the Θ_1 test than for the others. This occurs essentially because of the above property that the Θ_1 test is superior when there is only one nonzero population noncentrality parameter, which is always the case when testing pairs of means on individual variables. Thus if interest centers on maximum resolution of the group means on the different variables, rather than the most powerful test of the null hypothesis, the Θ_1 test is optimal. Gabriel has considered a six-group, three-variable example in great detail, carrying out an STP on all subsets of groups and all subsets of variables using the largest root test.

Unfortunately, the question of the relative importance of the response variables in MANOVA is not adequately answered by Gabriel's test procedure. It is quite possible, for example, for the groups not to differ in mean significantly on one variable, while the largest root statistic for the remaining $p-1$ variables is very much less than that for all p variables, indicating that the omitted variable provides additional discrimination among the groups in the presence of the other $p-1$ variables.

The problem of determining which variables may be eliminated in MANOVA parallels the corresponding problem in multiple regression, and the methods suggested for it are similar. The two-group example analyzed by Dempster above converts the Hotelling's T^2 problem into a regression problem with a dummy criterion variable. The significance of individual discriminant function coefficients may then be assessed by t tests analogous to those on regression coefficients. These suffer from the same disadvantages as those in regression. Rao (53, p. 467) has given a test for "additional information" which tests whether a *specified* set of discriminant function coefficients is zero. However, one will generally not know in advance which variables are to be omitted as possibly irrelevant. J. Roy's stepdown F test (56) tests sequentially whether each variable contributes further discrimination beyond those already included, but it suffers from the same disadvantage as analogous stepdown tests in multiple regression: the order of variables to be included must be specified in advance of the data analysis. Again this will generally not be possible.

The relative merits of various methods of choosing subsets of variables have been examined by Weiner & Dunn (68). From a set of 25 medical

variables in five distinct two-group problems, subsets of two, five, and ten were chosen by each of four methods:

(a) the two, five, and ten variables with the largest two-sample Student's t ; (b) the two, five, and ten variables with the largest standardized discriminant function coefficients; (c) the two, five, and ten variables first entered into a stepwise regression program; (d) two, five, and ten variables chosen at random.

The chosen variables were then used to calculate discriminant functions based on two samples of 50 each, and these were used to classify new samples from each population in each problem. It is surprising to find that method (b) was much poorer than methods (a) and (c), and only marginally better than random selection! It seems to me, however, that the restriction to a fixed number of variables misses the point of these methods of eliminating variables, which is to arrive at some minimal set which contains essentially all the discrimination available from all the variables. If the minimal set contains more than two, five, or ten variables, then restriction to two, five, or ten will not give a true indication of the relative merits of the different methods.

In an unpublished paper, R. J. McKay has approached the problem using a simultaneous test procedure. In the general one-way classification in MANOVA, he considers the differences among groups in terms of the roots of the noncentrality matrix

$$\sum_{i=1}^k n_i(\mathbf{u}_i - \bar{\mathbf{u}})(\mathbf{u}_i - \bar{\mathbf{u}})' \Sigma^{-1}.$$

For each subset of variables, he tests simultaneously whether the number and size of the nonzero roots of the reduced noncentrality matrix using the subset of variables are the same as those for the matrix using all variables. In the two-group problem, for example, this procedure sets a lower bound to the values of T^2 using any subset of variables, and any subset with a value of T^2 at least as large as the lower bound provides essentially the same discrimination between groups as does the original set. In the general case, the method is a generalization of both Roy's stepdown test and Rao's test for additional information, in that neither the variables to be eliminated, nor their order, has to be specified in advance. The method may also be applied to all subsets of groups, and is an STP in Gabriel's sense.

A similar idea has been used by Grizzle & Allen (26) in the covariance analysis of a growth curve model. Repeated measurements at equally spaced time intervals are replaced by orthogonal polynomials. Those higher order polynomials not required to describe the growth curve are then examined for possible use as covariates, and are used if they have substantial correlation with the lower order polynomials required to describe the growth curve. The authors note that the probability of a Type I error will no longer be α since the covariates are specified by the data, not in advance of it, but conjecture that the power of the test for treatment effects on the growth curve will in-

crease. Since the size must also increase, it is not clear that the increased power will outweigh the increased size, which is unknown in any case. An STP in Gabriel's sense could be obtained by proceeding as though the *maximum* number of covariates were being used.

Evans & Anastasio (15) have cleared up some of the confusion over the use of analysis of covariance when the covariate is affected by treatments. They point out that its common use to remove variation in the response variable due to correlation with the covariate depends crucially on the assumption of within group homogeneity of regression, i.e. the regression of response on covariate must be the same in the treatment groups. This will be the case if the covariance matrices of response and covariate are homogeneous. It is also essential that the covariate does not differ in mean across the treatment groups, which is usually achieved by random assignment of experimental units to the groups.

This can be seen in the simple case of a two-group problem with one response Y and one covariate X . If Y and X have a bivariate normal distribution, then the conditional distribution of Y in the i th group, given $X = x$, is normal with mean $\mu_{Y_i} + \beta_i(x - \mu_{X_i})$ and variance $\sigma_{Y_i}^2(1 - \rho_i^2)$, where $\beta_i = \rho_i\sigma_{Y_i}/\sigma_{X_i}$ is the regression coefficient of Y on X in the i th group. If the covariance matrices are homogeneous and X has the same mean in the two groups, then the conditional means of Y in the two groups differ only in the term μ_{Y_i} , and the conditional variances are equal, so that a two-sample t test on the Y means adjusted for regression on X is a test of the hypothesis $\mu_{Y_1} = \mu_{Y_2}$. The reduction in variance from σ_Y^2 to $\sigma_Y^2(1 - \rho^2)$ gives increased precision if ρ is at all sizable, more than compensating for the loss of one degree of freedom in the within group mean square.

If, however, the covariance matrices are not homogeneous, so that $\beta_1 \neq \beta_2$, or if $\mu_{X_1} \neq \mu_{X_2}$, then the conditional means of Y do not differ only in the terms μ_{Y_i} , and the above t test will not be a valid test of the hypothesis $\mu_{Y_1} = \mu_{Y_2}$. If homogeneity of covariance matrices is maintained, but $\mu_{X_1} \neq \mu_{X_2}$, then the t test will be a test of the null hypothesis $\mu_{Y_1} - \mu_{Y_2} = \beta(\mu_{X_1} - \mu_{X_2})$, i.e. in a sense, that Y provides no further information than that provided by X about differences in mean between the groups. This test is, in fact, identical to a test for a zero discriminant function coefficient for Y in the two-group discriminant function using both variables, a simple case of Rao's test for additional information.

This test has been interpreted as a test for the difference in Y means after "adjustment" for the difference in X means. Evans & Anastasio point out that this interpretation cannot be used to "correct" for the difference in X means, i.e. the adjusted mean difference in Y cannot be assumed to be an estimate of the conditional mean difference in Y if there were no difference in X means. If, for example, the two groups are racial groups, Y is an IQ score and X a socioeconomic score differing in mean in the two groups, analysis of covariance may be used to test whether the groups are differentiated by IQ score "adjusted" for socioeconomic score, but if no difference is found, it

cannot be claimed that samples from the two racial groups selected to have equal mean socioeconomic scores would show no significant difference in IQ score. This could only be determined by actually carrying out the experiment, for the populations involved will be different from the unselected populations considered in the original analysis.

Williams (71) has considered the problem of testing whether a hypothetical set of canonical variables is consistent with the sample canonical variables in MANOVA. In general, it will be even more difficult to specify the linear functions of variables used as the hypothetical canonical set than to specify the important variables, so that this situation will not often occur. In fact, in the example considered by Williams, the canonical variables taken as the "hypothetical set" were suggested by an inspection of the sample canonical variables. Williams states (p. 203) that this does not invalidate the probability statements made, for "in principle we should examine all possible sets of discriminators . . . showing no significant departure from the data; the sets of canonical variates provide a useful guide in this task." But if "in principle" all possible sets are to be examined, the distribution theory for the particular set chosen after inspection of the data cannot be based on the assumption that this is a *specified* set: an STP is clearly required.

Levy (43) has given a formula for the probability of misclassification in a two-group problem. His formula is essentially that obtained by substituting the sample estimates $\bar{x}_1$, $\bar{x}_2$, and S for the corresponding parameters in the probability of misclassification when these parameters are known (see Anderson, 3, pp. 135-36). It is well known that this method considerably underestimates the true probabilities of misclassification. Lachenbruch & Mickey (41) have examined the performance of various methods for estimating probabilities of misclassification conditional on the sample statistics; the unconditional probabilities of misclassification remain analytically intractable. Although no one method proved consistently best, one method performing well in general was based on a jackknife procedure: each observation was in turn omitted from the sample, the discriminant function based on the remaining observations computed, and the omitted observation classified. This method may be considered an approximation to classifying an independent sample; the latter method was not investigated by Lachenbruch & Mickey because of the waste of observations involved, in that part of the sample has to be retained for classification and cannot be used for estimating the discriminant function.

The determination of the dimensionality of the subspace spanned by the mean vectors in MANOVA is an important problem, especially in multiple discriminant analysis, where the dimensionality defines the number of discriminant functions required to discriminate among the groups. The dimensionality is simply the number of nonzero roots of the noncentrality matrix,

$$\sum_{i=1}^k n_i (\bar{\mathbf{u}}_i - \bar{\mathbf{u}})(\bar{\mathbf{u}}_i - \bar{\mathbf{u}})' \Sigma^{-1}.$$

If there is only one nonzero root, for example, then the means are collinear and one discriminant function is sufficient. Tests for dimensionality have been given by Bartlett (5) and Rao (53, p. 474). Bartlett's test uses the statistic

$$\left(n - \frac{p+k}{2}\right) \sum_{i=r+1}^s \log(1 + \Theta_i)$$

where

$$n = \sum_{i=1}^k n_i - k,$$

which is distributed approximately as $\chi^2_{(p-r)(k-r-1)}$ if r population roots are nonzero. This test is related to the W test. Rao uses the statistic

$$n \sum_{i=r+1}^s \theta_i$$

which has the same approximate distribution as Bartlett's statistic, and is related to the T_0^2 test. Both these tests proceed sequentially, starting with $r=0$. Pillai's statistic presumably could be treated in the same way. The largest root statistic does not seem to have been proposed for this problem, but since the largest root critical value is appropriate for all contrasts of group means on all linear functions of variables, it is appropriate in particular for the contrast on the linear function of variables corresponding to the second largest, or any other, root. Thus *all* roots may be simultaneously tested for departure from zero by comparison with the percentage point of the *largest* root. These procedures are not commonly employed; an example will illustrate their use. Morrison (46, pp. 169-70) has analyzed the famous iris example on which Fisher's discriminant function was first used. Samples of 50 were taken from each of three species of iris, and four measurements made. The two nonzero roots of AW^{-1} were 32.188 and 0.285 (two roots are zero). Bartlett's test gives, for the null hypothesis of all population roots zero ($r=0$),

$$(147 - 3.5)(\log 33.188 + \log 1.285) = 502.56$$

compared with χ_8^2 , which rejects the null hypothesis. For the hypothesis of one nonzero root, the statistic is

$$-143.5 \log 1.285 = 35.98$$

compared with χ_3^2 . Again the hypothesis is rejected.

For comparison, Rao's test gives the corresponding values

$$147 (32.188 + 0.285) = 4,773.53$$

and $147(0.285) = 41.90$. Clearly the correspondence between $-\log 1/(1+\Theta)$ and Θ is poor if Θ is not small.

For the largest root test, the critical value for $\Theta/(1+\Theta)$ is, from Morrison's charts, 0.11 at the 1 percent level. Both values exceed this. All three tests lead to the conclusion that there are two nonzero population roots, so that the means do not lie in any smaller dimensional space. Two discriminant functions are required to completely characterize the positions of the means.

It is worth noting that the χ^2 distributions proposed for the Bartlett and Rao tests hold approximately only if the nonzero roots already tested are very large (corresponding to canonical correlations near unity), as pointed out by Williams (71, p. 220). Further, the Bartlett and Rao tests are not STPs, and can lead to logical difficulties as in the *NK* method. For example, if, in a three-group bivariate case with $n = 50$, the roots of AW^{-1} are $\Theta_1 = 0.10$ and $\Theta_2 = 0.09$, then Bartlett's test gives a value of 8.621, not significant at the 5 percent level of χ^2_4 , for both roots, and a value of 4.093, significant at the 5 percent level of χ^2_1 , for the smaller root. Rao's test similarly gives values of 9.50 and 4.50, the first barely significant. The largest root test, being an STP, cannot show this effect. The largest root critical value for the above example is 0.1625 at the 5 percent level, so the null hypothesis is not rejected.

PRINCIPAL COMPONENT AND FACTOR ANALYSIS

The distribution theory for the principal components of a sample covariance matrix is now more or less complete. Anderson (4) has developed the asymptotic theory for the sample characteristic roots and vectors of the covariance matrix when the population covariance matrix has roots of arbitrary multiplicity. Tests of hypotheses of equality of any set of population roots are given, and confidence intervals for assumed equal roots. The exact distributions of largest and smallest roots have been investigated for the case $\Sigma = I$, and some percentage points tabulated, by Hanumara & Thompson (28).

The corresponding theory for the principal components of a correlation matrix is still not complete. There are two problems of interest: the determination of the number $p-q$ of roots of the population correlation matrix P corresponding to error variance, in the representation $P = \Psi + \sigma^2 I$, where Ψ is the true score covariance matrix of rank $q < p$ and σ^2 the error variance; and the determination of the number of roots of P greater than unity.

For the first problem, a test was given by Bartlett (6) for the case $q = p-1$, corresponding to all roots equal to $1-\rho$ except the largest, equal to $1+(p-1)\rho$, P then being a matrix of constant correlations ρ . Other tests for this hypothesis have also been proposed by Anderson (4), Lawley (42), and Aitkin, Nelson & Reinfurt (2). Gleser (22) examines the three tests proposed and concludes that Lawley's is preferable. For the case of general q , the test proposed by Bartlett (6) and examined by Lawley (42), corresponds to a partition of the likelihood ratio statistic for the test of complete independence (i.e. all population roots equal to unity). The test statistic is

$$n \{ -\log (l_{q+1} l_{q+2} \cdots l_p) + (p - q) \log \{ (l_{q+1} + l_{q+2} + \cdots l_p) / (p - q) \} \}$$

where $l_1 > l_2 > \cdots > l_p$ are the ordered roots of the sample correlation matrix R . For $q=0$, this reduces to

$$-n \log (l_1 l_2 \cdots l_p) = -n \log |R|$$

which is the usual statistic for testing independence. The distribution of the above statistic is very complicated, and is not well described by a χ^2 distribution unless the population roots $\lambda_1, \cdots, \lambda_q$ are very large. Lawley approximates the distribution by a χ^2 whose degrees of freedom is determined by the mean of the statistic. In general, the degrees of freedom depend on $\lambda_1, \cdots, \lambda_q$.

The number of population roots greater than unity was proposed by Guttman (27) as an upper bound for the number of components corresponding to meaningful population variation. In practice, many users of principal component analysis have extracted all the components with *sample* roots greater than unity. Horn (32) has pointed out that because of sampling variation, too many components will be extracted. This has also been noted by Dempster (12) and others, in that the larger sample roots are upwardly biased as estimators of the corresponding population roots. Unfortunately, no test is available for determining whether a sample root is significantly larger than unity. Such a test would require the distribution of the largest root of the correlation matrix in the null case. The exact distribution theory of the roots of R , even in the null case, does not seem to have progressed beyond the joint distribution of the sample correlations given in Anderson (3, p. 175). In the absence of the necessary sampling theory, Horn (32) performed some sampling experiments with a null population correlation matrix. With $p=65$ and $n=297$, he found that the first 30 sample roots exceeded unity, the largest being 2.10. He suggested that in a principal component analysis of real data, a corresponding set of artificial data from an uncorrelated population be generated, and the sample roots from the artificial data be subtracted from the corresponding roots from the real data. The number of reduced roots of the real data matrix greater than zero could then be identified as the number of components to be extracted. This procedure, applied to a real set of $p=65$ measurements on $n=297$ individuals, resulted in the reduction of the number of components extracted from 16 to 9.

It is clear that this is not a valid test, since it amounts to declaring that a sample value is not consistent with a population value if it exceeds another random sample value.

Horn (33) also brings the concept of simple structure into question, in rotating the random components described above to a prespecified pattern using a Procrustes rotation due to Hurley and Cattell. This produced an acceptable simple structure with satisfyingly high loadings, and the rotated factors could be readily interpreted when the experimental variable names were used to label the random variables.

An allied problem is that of the significance of the loadings in a principal component analysis. Some large sample results are available for the analysis of the covariance matrix (see Morrison, for example), but not for the analysis of the correlation matrix.

A significant contribution to maximum likelihood factor analysis has been made by Jöreskog. In a series of papers (34–36) he has developed a general procedure in which any number of parameters in the factor analytic model can be held fixed at any values and the remaining free parameters estimated by maximum likelihood. This makes it possible to deal with any mixture of orthogonal and oblique solutions; and by choosing the fixed parameters appropriately, factors can be defined to have desired properties, so that rotation is unnecessary. Large sample χ^2 tests are also provided for goodness-of-fit, but the author notes in (35) that it is highly desirable for a hypothesis generated by an explanatory procedure to be confirmed or disproved by obtaining new data and subjecting these to a confirmatory analysis. Convergence difficulties in the maximization of the likelihood function which occurred with the Newton-Raphson method are avoided by using a steepest descent procedure.

NONPARAMETRIC METHODS

Several useful books have been published in this area. The book by Noether (47) covers the most important nonparametric techniques at a fairly advanced level, with much valuable material on distribution free confidence intervals. He notes in the introduction that in nonnormal populations the sample mean may be a very bad estimate of the center of symmetry of a distribution, and true confidence coefficients may differ considerably from nominal confidence coefficients suggested by normal theory.

The book by Bradley (9) goes much further in defense of distribution free procedures, making a frontal attack on the belief in “quasi-universal quasi-normality and quasi-homogeneity.” Bradley’s first chapter on the development of normal theory techniques makes entertaining reading, and he also makes several important points that are often neglected (p. 9):

It does not follow logically that approximate normality and homogeneity insure approximate validity of a test which assumes exact normality and exact homogeneity. . . . In the mathematical studies of violation of assumptions the populations to be sampled appear to have been chosen more for their mathematical convenience than for their prevalence in practice. . . . The more nonnormal of the artificial populations appear to have been regarded in some quarters as incorporating a ‘degree’ of nonnormality as extreme as any likely to be encountered in practice, and the results of studies based upon them appear to have been regarded as ‘bounds’ for the effects of nonnormality upon the test statistic.

The “Myth of Robustness” comes in for severe criticism in Chapter 2. The most insidious thing about the Myth of Robustness, however, is that the ‘degree’ of a test’s robustness against violation of a given assumption is *strongly* dependent upon factors which are not involved in the statement of a test’s as-

sumptions, which are often not required in a complete description of the assumption's violation, and which are not mentioned in the usual allegation of robustness. . . . These factors cause no distortion of Type I or Type II errors when all assumptions are met, but greatly influence the distortion occurring under a given violation of assumptions—i.e. the factors interact with whatever violation occurs. (9, p. 26)

Such factors are location of the rejection region, size of the region (i.e. α level), sample sizes, minimum sample size, relative shapes of populations, correlation between mean and variance, and so on.

The most compelling evidence against robustness is that provided by the author's sampling studies using samples generated from an artificial population, closely resembling a real population of 2520 time scores for the operation of a pushbutton. The artificial population was a mixture of normal populations in the ratio of about 8 to 1. The larger population had mean 96 and standard deviation 3, the smaller had mean 130 and standard deviation 8. For example, for samples of $n=32$ drawn from this population, the left tail probability corresponding to the $\alpha=0.05$ point of $Z=(n)^{1/2}(\bar{X}-\mu)/\sigma$ was 0.03, and for $\alpha=0.025$ was 0.00375. Comparably poor results were obtained for the two-sample t test with $n_1=n_2=8$. Both these tests are claimed to be very robust against non-normality.

Having disposed of normal theory procedures in the first two chapters, Bradley devotes the rest of the book to a comprehensive development of distribution free tests, classified according to the purpose of the test and the family to which the tests's derivation belongs. Extensive tables and references are given, and the book is at least as useful as Siegel's (65), though the latter is at present being revised.

More attention is now being paid to the teaching of introductory statistics using nonparametric procedures. The book by Hodges & Lehmann (31) omits the t test entirely, replacing it by the Wilcoxon two-sample and signed rank tests. Kraft & van Eeden (39) use nonparametric methods right at the beginning to allow an introduction to inference before a discussion of probability. Unfortunately, their algebraic notation is discouraging and the early chapters are too dry to provide much motivation. Noether's book (48), to be published in 1970, goes much further in eliminating algebraic difficulties encountered by students in introductory statistics courses. The sample standard deviation, for example, is never introduced, and the population standard deviation is required only for the normal approximation to the binomial. The sign test, median test, and the two Wilcoxon tests replace the t test. Confidence intervals for the median based on order statistics replace the usual confidence intervals for the mean based on the t test. This approach to inference allows the probability arguments to be very simple since they are based only on simple random sampling. The more conventional analysis of variance procedures can then be developed in a second course, after the student has achieved a solid foundation in the principles of statistical inference.

Important new rank methods have been developed by Puri & Sen (20, 51, 52, 60, 61). They have developed rank analogs of many of the standard normal theory tests which are strictly distribution free and have simple asymptotic distributions. An important example is the MANOVA analog described in (51). This is especially useful because of the stringency of the assumption of multivariate normality. As Puri & Sen's paper is very difficult to read, and no example of the technique is given in their paper, an example is given here.

Consider a MANOVA one-way classification with k groups and n_j observations in the j th group. A rank test for the null hypotheses of k identical populations may be carried out as follows. For each variate, the

$$N = \sum_{j=1}^k n_j$$

observations are ordered, and the rank of $X_{i\alpha}^{(j)}$, the α th observation in the j th group on the i th variate, is denoted by $R_{i\alpha}^{(j)}$, $\alpha = 1, \dots, N$. Denote the mean rank in the j th group on the i th variate by $\bar{R}_i^{(j)}$, so that $\bar{R}_i^{(j)} = \sum_{\alpha} R_{i\alpha}^{(j)}$, where the α summation is over the range

$$\alpha = n_1 + n_2 + \dots + n_{j-1} + 1 \quad \text{to} \quad \alpha = n_1 + n_2 + \dots + n_{j-1} + n_j.$$

The mean rank over all groups on the i th variate will be $\bar{R}_i = \bar{R} = (N+1)/2$ for all i .

A MANOVA is now carried out using the ranks of the observations as though they were the observations themselves: let $A = (A_{il})$ be the "among group" matrix of sums of squares and cross products of the rank scores, so that

$$A_{il} = \sum_{j=1}^k n_j (\bar{R}_i^{(j)} - \bar{R}_i) (\bar{R}_l^{(j)} - \bar{R}_l) = \sum_{j=1}^k n_j \bar{R}_i^{(j)} \bar{R}_l^{(j)} - N \bar{R}^2$$

and let T be the "total" matrix, so that

$$T_{il} = \sum_{j=1}^k \sum_{\alpha=1}^{n_j} (R_{i\alpha}^{(j)} - \bar{R}_i) (R_{l\alpha}^{(j)} - \bar{R}_l) = \sum_{j=1}^k \sum_{\alpha=1}^{n_j} R_{i\alpha}^{(j)} R_{l\alpha}^{(j)} - N \bar{R}^2.$$

Then Puri & Sen show that $N \operatorname{tr} AT^{-1}$ is asymptotically distributed as $\chi_{p(k-1)}^2$ under the null hypothesis of identical populations. For the univariate case $p = 1$, this reduces to the Kruskal-Wallis test. This statistic is a direct analog of Pillai's V criterion in the normal case; presumably other functions of the roots of AT^{-1} could also be used. The characteristic vectors of AT^{-1} corresponding to each root give the relative importance of the variables in that root; presumably simultaneous test procedures of the kind developed by McKay in the normal case could also be applied here, although the theory has not been developed.

Consider a bivariate example of the above test in the two-group case.

Suppose we have $n_1=4$ observations in the first sample and $n_2=6$ in the second. The observations are

$$(20.1, 74.4), (94.7, 22.1), (93.2, 45.0), (44.9, 16.2)$$

and

$$(4.5, 32.7) (3.6, 62.4) (61.0, 89.0), (1.7, 27.5), (49.0, 49.7), (20.2, 48.8).$$

Ranking the observations on each variate gives the rank scores

$$(4, 9), (9, 2), (10, 5), (6, 1)$$

and

$$(3, 4), (2, 8) (8, 10), (1, 3), (7, 7), (5, 6).$$

Mean rank vectors for the two samples are

$$(7.25, 4.25) \quad \text{and} \quad (4.33, 6.33)$$

and the total matrix is

$$T = \begin{pmatrix} 82.25 & -2.5 \\ -2.5 & 82.25 \end{pmatrix}$$

with inverse

$$T^{-1} = \begin{pmatrix} 0.012,169 & 0.000,370 \\ 0.000,370 & 0.012,169 \end{pmatrix}$$

The among sample matrix is

$$A = \begin{pmatrix} 20.417 & -14.583 \\ -14.583 & 10.417 \end{pmatrix}$$

so that $N \text{ tr } A T^{-1} = 3.698$, corresponding to a one-tail probability of 0.157 for χ_2^2 (in practice the exact permutation distribution would be needed for samples of this size but it has not been tabulated). The discriminant function is proportional to (0.035, -0.024). Note that two sample Wilcoxon tests on the variables separately give two-tail probabilities of 0.172 and 0.352.

A similar procedure is given by Bennett (8) in constructing a rank test for the general linear hypothesis and subhypothesis. The observations on the dependent variable are replaced by ranks, and a formal multiple regression of the ranks on the x variables is carried out. Bennett shows that the estimator of β obtained is a first approximation to the nonlinear least squares estimator of β . Analogous decompositions of the total sum of squared ranks are obtained, and χ^2 tests are proposed for the null hypothesis $\beta=0$ and for a subhypothesis $\beta_2=0$. The tests reduce in appropriate cases to the Kruskal-Wallis, Wilcoxon, and Spearman's rank correlation tests.

These rank procedures have the usual properties of such procedures—simplicity (though matrix inversion is still required above), completely distribution free with exact null distributions which require enumeration for small sample sizes and simple asymptotic distributions, high asymptotic efficiency relative to normal procedures when normal assumptions are valid, considerable gain in efficiency under non-normal conditions, and insensitivity to outliers. The systematic development of these procedures should produce important shifts in emphasis in the use and teaching of statistics.

TESTS FOR NORMALITY

An important new development in this area is due to Shapiro & Wilk (63, 64, 70). They have proposed a new W test for normality, derived in spirit from probability plotting considerations. For a single sample of size n , if the ordered sample values are $y_1 \leq y_2 \leq \dots \leq y_n$, then the W statistic is defined by

$$W = \left[\sum_{i=1}^k a_{n-i+1} (y_{n-i+1} - y_i) \right]^2 / \sum_{i=1}^n (y_i - \bar{y})^2$$

where $n = 2k$ or $2k+1$, for n even or odd, and the a_i are the expected values of normal order statistics in a sample of size n . Values of the a_i , and percentage points of W , are tabulated in (63) for $n \leq 50$. The W statistic is location and scale invariant, so the parameters of the normal population do not have to be estimated. Power comparisons of the W test with eight other tests for normality [including the χ^2 goodness-of-fit (b_1)^{1/2} and (b_2)^{1/2}, Kolmogorov-Smirnov (KS), and Cramer-Von Mises tests] show that the W test is generally superior over a wide range of non-normal populations. This superiority is even greater than indicated by these results, however, as the mean and variance of the population are taken as known in all cases, though this will rarely be the case in practice. The use of the KS test, for example, requires that the population mean and variance be known. Lilliefors (44) has pointed out that if this test is used with estimated parameters, the test will be extremely conservative, so that the W test, which does not require this specification, will be even more powerful than in the cases considered by Shapiro & Wilk. Lilliefors gives percentage points of the KS test based on Monte Carlo sampling for a normal population with estimated parameters. The KS test is no longer distribution free in this case.

In (64, 70) Shapiro & Wilk extend the W test to the case of several independent samples (as in ANOVA), and examine approximations to the distribution of W for large n .

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PSYCHOLINGUISTICS¹

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Some idea as to the profusion of work potentially relevant to the area of psycholinguistics may be obtained if one notes that *Language and Language Behavior Abstracts*, which have been published since 1967, have run to over 4000 pages in 3 years, and that at an average of nearly three abstracts per page this amounts to a total of over 10,000 items. Much of the material cited is, of course, tangential at best, but much is pertinent. As another indication of interest in psycholinguistics, consider the fact that there have been something of the order of a dozen major conferences in a period of roughly 5 years (29, 40, 51, 54, 58, 67, 88, 100, 123, 163, 168, 172, 189, 209). The proceedings of these conferences have yielded many very important papers; taken together they give a pretty good idea of the status quo in the field.

HISTORICAL BACKGROUND

To begin with a historical note, once it seemed as though transformational grammar was the entire, new creation of Chomsky, and the psycholinguistic work influenced by this position was also regarded as something very new, and different. Now, just as Chomsky has sought his intellectual roots in *Cartesian Linguistics* (35), so contemporary psycholinguists have been seeking their intellectual forebears. McNeill sees current debates regarding the nature of language and language acquisition as foreshadowed, if not completely formulated, in the controversy between Bishop Berkeley and Samuel Bailey (136). Neisser (153) has nominated Gestalt psychologists as ancestors of present-day psycholinguists because of their emphasis on structural properties and innate determinants of perception, their concern with internal knowledge as against the external exhibition of that

¹ The most recent chapter on psycholinguistics in the *Annual Review of Psychology* (64) reviewed work through June 1965. The present review covers the period from 1965 through the end of 1969. I wish to thank those who made reprints and especially preprints available to me. As yet unpublished materials of significance are noted as appropriate. Work on this review was supported in part by United States Public Health Service Grant MH10006 from the National Institute of Mental Health. I am indebted to Mr. B. J. Calder for his aid in assembling materials for this review.

knowledge, and their staunch opposition to behaviorist formulations. This attribution has been denied by Bever (13), who with Blumenthal (19) regards Wundt as the true predecessor of current work because of his claims that the study of language is a branch of psychology, that the basic unit of analysis is the sentence, and that underlying every sentence is a set of internal relations. Blumenthal (20), stressing two problems—"the distinction between form and content in language, and the question of whether word or sentence is to be the fundamental reference point in the study of language"—has contributed a very valuable review of early work in psycholinguistics with particular attention to language performance effects, work on language acquisition, and work in the psychology of reading. He has organized an even more useful history of psycholinguistics, emphasizing developments in psycholinguistic theory with particular focus on the psychology of the sentence (19), tying together selections from judiciously chosen authors with extensive interpretative commentary. For a rather different perspective on the psychological background of linguistic theorizing, and particularly the development of the early behaviorist account of the psychology of language, see Esper (65).

Turning to the most recent history of psycholinguistics, considering the last half dozen years or so, it is clear that work in linguistic theory or more particularly work on generative transformational grammar has had a continuing and growing impact in substantive ways, leading, for example, to new theoretical perspectives on language acquisition (see e.g. McNeill 132) and to much experimental work seeking to establish the psychological reality, at least, of some of the linguistic structures described by the linguists, and inquiring as to the psychological status of linguistic rules (see e.g. 73, 197). There has also been what can best be called an ideological impact, symptoms of which may be seen in the conversion of such distinguished experimentalists as Jenkins and Deese, and in their current views regarding limitations on what can be done with experimental techniques and evidence (56, 96), a conservatism that leads Deese almost to abjure the use of experiment. In very recent time there has also developed a subtler appreciation of the complexities of the relations between any linguistic formulation and any psychological account (see e.g. 13, 71, 200). Finally, one should note that there have been very significant developments in linguistics proper, even, or particularly, among transformationalists.

THE RELATION BETWEEN LINGUISTIC AND PSYCHOLOGICAL FORMULATIONS

How to conceive of the relation between psychology and linguistics constitutes perhaps the most critical generic problem for the student of the psychology of language as he worries about the ways or extent to which linguistic analyses may control the design and interpretation of psychological research (see e.g. 175), and whether the results of such research, in turn, can have any implications for linguistic formulations. The problem

has come to be posed in terms of the relations between competence and performance models. What might be termed the standard doctrine is stated very clearly by Chomsky, who claims that linguistics can best be conceived of as an (advanced) branch of cognitive psychology:

Linguistic theory is concerned primarily with an ideal speaker-listener in a completely homogeneous speech-community who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance . . . A grammar of a language purports to be a description of the ideal speaker-hearer's intrinsic competence [knowledge of his language] . . . a generative grammar is not a model for a speaker or hearer . . . we say nothing about how the speaker or hearer might proceed . . . these questions belong to a theory of language use . . . the theory of performance. No doubt a reasonable model of language use will incorporate as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of the language; but this generative grammar does not, in itself, prescribe the character or functioning of a perceptual model or model of speech production (34).

Granting for the moment that some distinction along the above lines must be made between knowledge and its contingent enactment, there arise immediately questions as to (a) what may be the most appropriate linguistic model to use, and (b) what one might mean by "incorporate" above, and what sorts of psychological assumptions to make regarding the performance model, and the ways in which this model will be constrained by (a).

As to a linguistic model, shall it be a phrase structure model of the sort presented by Yngve (208), a stratificational model in the spirit of Lamb (162), a transformational model after Chomsky (34, 152), or what, and what difference will a particular choice make? Due principally to the tutelage of George Miller, psycholinguists have been most influenced by Chomsky's formulations. But of course there have been significant changes in Chomsky's views over time. As between 1957 (33) and 1965 (34) there were important changes—for example, in the later work a level of "deep" syntactic structure serves an essential role in the analysis, and transformations are regarded as meaning preserving, with semantic interpretation a function of deep structure only. There have been changes in Chomsky's views since then; for example, surface structure now is regarded as relevant, in some cases at least, to semantic interpretation (38). And it is no secret, even to psychologists, that various linguists (see e.g. McCawley 130) have argued that "deep" structure of the sort postulated by Chomsky is just not deep enough, that indeed there is no warrant for assuming an autonomous level of deep structure, that semantics must be generative not simply interpretive, that semantic and syntactic representations are essentially of the same formal nature, and that (a somewhat enriched) symbolic logic constitutes the most appropriate general framework for the representation of meaning.

For a response to these arguments, in part questioning whether they are in fact substantially different from the standard position or represent only notational variants, in part attempting to provide a rebuttal, see Chomsky (38). While at this point it is not at all clear what the implications of these new formulations are for the work of the psycholinguist, one general point must be quite patent. To the extent that psycholinguistic work is based on some linguistic formulation, it may be embarrassing, or likely much worse, to find that linguists have now rejected that formulation, making very difficult indeed the interpretation of any results.

What might be meant when it is said that any model of language must "incorporate" a competence model will be discussed directly below, when we consider the relation between competence and performance models. In the course of such a discussion we shall necessarily be concerned with the constraints that the former imposes upon the latter. With regard to most attempts at (fragments of) a performance model, one thing is rather striking, —there has been very little recourse to what is generally known about attentive mechanisms, short term and longer term memorial processes, temporal constraints on information processing, etc. Whether because such materials were unknown to the investigators, or whether they were deemed irrelevant is not clear. What is clear is that such performance model building as there has been has been relatively distinct and separate from other work in perception, cognition, and learning [for a very immediate appreciation of this chasm or gap note how conceptually segregated most of the verbal learning papers are from the psycholinguistic papers in (58)].

How to conceive of the relation between competence and performance will depend in some measure upon the way in which these have been defined, and in part on empirical considerations as one tries to connect various properties of the competence model with findings regarding performance. Thus to say anything on this at the present point will be, in part, to anticipate some of the findings to be reported later on; nevertheless it is necessary to consider this now in order to provide some orientation or perspective for these findings.

Given the very influential phrasing of the distinction between competence and performance models by Chomsky, mentioned above, how might one formulate the relation between these models? One possibility might be what has been called the correspondence hypothesis, viz that the "derivational history of the sentence corresponds step by step to the sequence of psychological processes which are executed when a person processes the sentence." While perhaps the simplest of all possibilities, it also seems very implausible stated in such naked, stark form. A slightly weaker hypothesis which has been called the derivational theory of complexity argues that performance complexity or degree of difficulty in processing a sentence should be positively related to competence complexity or the length of the transformational derivation of a sentence. Something like this hypothesis motivated much of the early experimental work which sought to demon-

strate a direct correspondence between "formal features of derivations, like length in rules and perceptual complexity . . . [and] the early results were a most impressive series of successes" (73). However, there are strong reasons for calling this hypothesis into question, both in terms of a re-analysis of the evidence that appeared to support it, and in terms of a consideration of further evidence incompatible with the hypothesis (see 13, 73, 200). A more circumspect hypothesis which Fodor (71) has called the "weak psychological reality" position

claims that insofar as the grammar assigns the correct structural description to sentences, it *ipso facto* specifies the structures that the performance model must recognize and integrate. But . . . the grammar is neutral with regard to the character of the psychological operations involved in sentence processing. In effect the weak view claims psychological reality for the structural descriptions enumerated by a grammar but for nothing else.

It is claimed "essentially that the grammar of 'English' is an axiomatization of the idealized speaker's mental grammar: that is, the two grammars have an exceedingly close relation"; indeed "that it is the *full* structural description of a sentence which is the psychologically pertinent output of a recognition device" (73). On this view, which perhaps comes closest to representing the current consensus, the job of the psychologist obviously must be to discover the strategies or heuristics employed, say by a listener, to extract such a structural description from the surface information available to him, and this is exactly what Fodor & Garrett and Bever have attempted to do (13, 74); we will touch on some of this work later.

Still more radical changes are to be found in some recent analyses due to Watt (200) and to Bever (14)—the former adduces strong grounds for doubting that the (idealized) mental grammar is a linguistic grammar in the usual sense, while the latter challenges the very distinction between competence and performance, noting that "a real grammar does not in fact describe an abstract linguistic world but rather a set of intuitions about 'grammaticality' held by native speakers" (14).

To this point the "essential business of the LG [linguistic grammar] was seen as accommodating in an ('idealized') MG [mental grammar] the linguistic information available to the ('idealized') native speaker" (200), the question then is simply whether "this abstract MG was in fact the LG." On the basis of "complexity discrepancies which do not appear reconcilable with any account of performance that is tied in a reasonable and consistent way" to a linguistic grammar, Watt concludes that the idealized grammar is not a competence grammar at all, but rather what he calls an APG (abstract performative grammar) which "puts a premium on economy of derivation of individual sentence paradigms" rather than on overall economy, thus missing some of the significant generalization possible in the linguistic grammar (200). (See Watt's review of some of the evidence, an assortment of *Gedanken* experiments and a consideration of the power,

or rather lack of power, of the strategies and heuristics suggested by Fodor & Garrett.) Watt puts the difference between his position and that of Fodor & Garrett very sharply, noting that "predicted discrepancies between LG and performance are explained in completely different ways: in Fodor & Garrett's account the discrepancies occur between MG (LG) and performance, whereas in my account . . . the basic source of these discrepancies occurs between LG and MG." And one must surely agree with Watt that this "is no mere terminological issue since the two hypotheses propound quite different notions about the nature of the MG, about the nature of the relation between MG and LG, and about an important aspect of the ontology of the rules being integrated into the MG" (200).

If "competence is performance," if the distinction between them is artificial in that even competence is a kind of performance, then

even if our linguistic intuitions are consistent there is no reason to believe that they are uniquely direct behavioral reflections of linguistic knowledge. The behavior of having linguistic intuitions may introduce its own 'performance' properties; that is, there is no guarantee that a linguistic grammar is either a *direct* or *ideal* representation of linguistic structure . . . a grammar is *not* necessarily a unique, basic 'nonpsychological' representation of linguistic structure; it is merely a description of the most direct and available of all behavioral reflections of real grammatical structure (14).

Thus the relation between, on the one hand, a linguistic grammar based on intuitions regarding sentencehood and structural relations among sentences and, on the other hand, descriptions of other kinds of language behavior may not just be 'abstract' or indirect in the sense of Fodor & Garrett "but may be *non existent* in some cases." Some linguistic intuitions may be a function of perceptual mechanisms and for others, as perhaps with regard to transformational relations, the very behavior of having "intuitions may introduce some properties which are *sui generis* and which appear in no other kind of language behavior." It is suggested that "*the formal description of a language using transformations depends on a form of intuitions which is irrelevant to most ongoing speech behavior.*" All this "forces us to reject as obvious the claim that a linguistic grammar is in any psychological sense internal to such linguistic performances as talking and listening" (14).

The foregoing has been presented at some length since it concerns critically differing versions of the nature of the relation between linguistic and psychological formulations. Differences in this regard involve crucial differences in perspective which may substantially affect orientations to research, and to the interpretation of the results of research.

BIOLOGICAL FOUNDATIONS AND THE PROBLEM OF UNIVERSALS

Does it make sense to study human language within the same framework as animal communication systems, or does human language have important *sui generis* properties that differentiate it from any such systems?

**ON THE CONTINUITY BETWEEN HUMAN LANGUAGE AND
ANIMAL COMMUNICATION**

The first alternative is taken by ethologists such as Thorpe (195) who point out that language shares with other animal communication systems such properties as purposiveness, syntactic organization, and informativeness (or propositionality). Hockett & Altmann, who have written about design features in human and animal communication, have now reformulated their analysis (93) in terms of a number of frameworks subsuming features relating to channel of communication, features of the social setting, features related to the behavioral antecedents and consequences of communicative acts, matters of continuity and change in communication systems, and features of repertoire and message for a single system, the intention being to make more coherent and orderly the previous listing of design features in all-or-none fashion, item by item.

Premack & Schwartz (161) seek to tackle the continuity problem, at least as between man and chimpanzee, in an experimental fashion, proposing to teach a language which is a sort of five-dimensional Morse code where auditory dimensions are associated with motor dimensions by use of a joystick apparatus, and where the first grammar to be taught would be "a phrase-structure grammar of a highly restricted type, e.g. context-free rules, minimal nesting, no self-embedding, no recursiveness." Those discussing the proposal, which is in many ways rich and sophisticated—e.g. in modeling an analogy to a distinctive feature account of phonology—are generally pessimistic as to the outcome (this includes even the authors). The crucial thing of course is what either success or failure of the enterprise might teach us—for example, would success in teaching a phrase structure grammar without any recursive properties have any implications for human language which cannot be handled by a phrase structure grammar and which crucially requires recursiveness? Would failure be specifically revealing or just another failure to teach chimps to talk? In a subsequent paper (160) Premack grants that "not only human phonology but quite possibly human syntax may be unique to man," but claims that basic functions of language may depend upon mechanisms of logic and semantics which may be more widely distributed. Again the chimpanzee is to be the "drawing board," and Premack will attempt to show how application of the appropriate training procedures will result in such linguistic functions as "word, sentence, question." The new language avoids problems of phonology and the auditory channel by using plastic "words," varying in shape, size, texture, and color, as its physical elements. While in the earlier paper no results are reported, in this paper Premack does give some illustrative results, suggesting e.g. that the animal can respond to some interrogatives, that some sorts of compound sentences with hierarchic structure could be appropriately handled, and that some sort of naming was taking place. As things stand, the results are still too fragmentary to evaluate, although the procedures employed are certainly a great credit to the ingenuity of the experimenter. However, one must surely have some theoretical pause when the author

claims e.g. that "the piece of plastic is a word when the properties ascribed to it are not those of the plastic but are those of the object designated by the piece of plastic." Since when is it a condition of naming that a word and its referent have the same properties? Surely naming and equivalence are not to be identified.

The Gardners (77) seek to eschew all theoretical speculation, arguing that if an animal can be taught a form of human language then the problem of continuity is settled *empirically*. Agreeing with Premack that chimpanzee vocalizations may not be very appropriate as a mode for communication, and noting the manual agility and dexterity of that animal, they decided to attempt to teach their subject American sign language, a system in which some signs are highly iconic, others quite arbitrary. Washoe (the chimp) "has been exposed to a wide variety of activities and objects together with their appropriate signs in the hope that she could come to associate the signs with those referents and later make the signs herself." The animal has acquired a substantial vocabulary, more than 30 signs by the twenty-second month of the project, there is spontaneous naming, signs have transferred to new members of each class of referents, and there has been an emergence of "rudimentary combinations of signs." It is this last phrase which is most tantalizing, for it gives a hint as to the possibility of the development of syntax.

Obviously it is too early to tell what the animal will achieve, but some important cautions voiced by McNeill (137) and Lenneberg (117) should be kept in mind. Uttering words in combination (or concatenation) is not the same as uttering sentences; it is not clear whether Washoe combines words in fixed order in something corresponding to grammatical relations, or whether she combines words without restriction. More generally it seems quite impossible to solve the problem of whether another species can use human language by empirical brute force, without any recourse to theoretical analysis. For as Lenneberg notes, "suppose one could show that what is being taught the animal only bears some tenuous and farfetched similarity to language, or that it is actually quite different in its most essential aspects, would that not make the 'gathering of data' a rather futile undertaking?" (117). Essentially the point is that data do *not* speak for themselves, loud and clear. Lenneberg then proceeds to argue that at least five different conditions must be met "in order to show empirically that that chimpanzee trained by the Gardners is acquiring even a primitive aspect of language" (these same conditions obviously hold also with regard to the work of Premack, or anyone else). One must assess the capacity or power of the communication system employed as used between men, one must test its efficiency as a communication system between man and chimpanzee, one must test the animal's knowledge or understanding of the language and its ability to deal with sentences in the language, one must be able to demonstrate the existence of simple language operations, e.g. the ability to cope with imperative and interrogative constructions, and one must demonstrate

productivity of understanding. A large vocabulary and concatenation of items are not sufficient criteria for the command of a language, rather it is concatenation according to rule or principle that is important. Obviously these considerations do not detract from the potential importance of the work by the Gardners and Premack, but only provide a firmer basis for the interpretation of the possible significance of such work. For a most provocative discussion of "the relation between human language and the signals that animals use or can learn to use" in the context of an examination of the work of the Gardners, see Bronowski & Bellugi (23). They attempt to specify some crucial properties of human language in terms of disengagement from "the immediate context," and particularly in terms of the ability to re-constitute language by extracting and resystematizing rules.

Many linguists and psychologists are skeptical as to the significance of similarities between human language and animal communication systems (see e.g. 36, 37, 115) and believe that human language may be a unique phenomenon without any significant analog in the animal world, and that the possession of human language is associated with a very particular type of mental organization consequent upon some specific aspects of human brain functioning. For example, Chomsky (36) points out that at that level at which human language and animal communication systems are propositional, syntactic, and purposive, so are many other things such as walking, and it is not clear that anything much is to be gained by lumping everything together, that in fact very striking differences appear between human language and other systems when one considers how the various systems are informative, structured, and purposive. As to propositionality or informativeness, every animal communication system either "consists of a fixed finite number of signals each associated with a specific range of behavior or emotional state" or "it makes use of a fixed, finite number of linguistic dimensions each of which is associated with a non-linguistic dimension in such a way that selection of a point along the linguistic dimension determines and signals a certain point along the associated non-linguistic dimension." But the devices used by human language are of an entirely different kind, and it does not make very much sense to analyze human language in the above terms. Further, it is not at all clear that the human use of language is characteristically informative or purposive in seeking to modify the behavior or thoughts of others, for language can be used, and often is, to all sorts of other ends. The beginning of wisdom here, Chomsky argues, is to look not to the uses of language but to its structure, "what it is."

THE NATIVISTIC THESIS

It is very much an open secret that generative linguists and psychologists influenced by them have taken a strongly nativistic stance, arguing that human language is a species-particular achievement which must be contingent upon a very specific biological endowment. For some critical comments on this thesis by philosophers, see many of the papers in Hook

(93a). In the main, the argument has been one from impotence or by exclusion (see e.g. 34, 108, 135). That is, given the productivity and the structural complexities of language as revealed by linguistic analysis and the (presumptive or demonstrated) inability in principle of any empiricist or learning formulation to account for these or their acquisition, it is concluded that the only way to account for the facts is to postulate strong built-in biological constraints or specific capacities for language. On this matter the event of recent years is the publication of Lenneberg's book, *Biological Foundations of Language* (115), which seeks to provide an independent basis for such a position directly in terms of a great variety of biological evidence (also see 116).² The basic thesis is that

language is the manifestation of species-specific cognitive propensities. It is the consequence of biological peculiarities that make a human type of cognition possible . . . which set strict limits to the range of possibilities for variations in natural languages . . . his [man's] entire cognitive function of which his capacity for language is an integral part is species-specific . . . a biological predisposition for the development of language is anchored to the operating characteristics of the human brain.

Reviewing a vast array of evidence with regard to morphological, psychological, neurological, and genetic aspects of language, work in language development, and on pathologies of language, Lenneberg comes to the conclusion that the capacity for development of language is species-specific. It is a consequence of maturation, with milestones of language acquisition interlocked with other maturational milestones (a synchrony frequently preserved even with dramatic slowdowns in the whole maturational sequence). He notes that special training cannot do much to force precocious language development, that there appears to be a critical period for language acquisition which is correlated with certain parameters of brain development—that neurological material strongly suggests that something happens in the brain during the early teens that changes the propensity for language acquisition (116), and that this may be connected in some fashion “with the peculiar phenomenon of cerebral lateralization of function.” It is impossible to summarize in any brief form such a rich assemblage of evidence and the arguments based on it, so here it will only be reiterated that Lenneberg's work constitutes the single most comprehensive attempt to provide information on the biological foundations of language, that at this time “the specific neurophysiological correlates of speech and language are completely unknown” (115), and that nothing at all can be said specifically as to the relation between some of the biological facts presented and assessed by Lenneberg and particular innate components of language of the sort postulated by linguists or psychologists.

² The Bems (12) applaud Lenneberg's book in so unlikely a place as the *Journal for the Experimental Analysis of Behavior*—a coup, surely!

LANGUAGE UNIVERSALS

If language is the manifestation of species-specific cognitive properties which set strict limits to variations between natural languages, then presumably, because of such a common biological foundation, all human languages ought to have some important, significant properties in common, i.e. there should be significant biologically based universals of language. Thus we may have a biological rationale or basis for universals of language (34, 85, 86, 152). Given that there are biologically founded universals of language, there arises the question of whether there is anything specifically linguistic about these, or whether they are just particular manifestations of some "species-specific cognitive propensities," as argued by Lenneberg above. On this score we find a Piagetian psychologist such as Sinclair de Zwart (181) agreeing with Osgood in his role of proponent of representational mediation theory (156), and both in agreement with Lenneberg in arguing that there need not be anything language-specific about linguistic capacities. Linguists such as Chomsky who regard language as a species-specific capacity that is essentially independent of intelligence, and who argue that the mastery of language at an early age requires formidable cognitive resources of a sort not otherwise evident in the behavior of the very young child, appear to be claiming that these capacities or abilities are uniquely linguistic. On the other hand, there is Chomsky's claim that "there is no better or more promising way to explore the essential and distinctive properties of human intelligence than through the detailed investigation of the structure of this unique human possession" (37), which seems to argue for an intimate relation between linguistic and other cognitive capacities. After some vacillation (see 132, 133), McNeill (137) has come to the position that, in general, one cannot say that there may be "weak linguistic universals" which have as a necessary and sufficient cause one or more universals of cognition or perception, and "strong linguistic universals" where universals of cognition may constitute necessary but not sufficient conditions, since uniquely linguistic capacities are also required (not to speak of "erratic" universals which may have more than one sufficient cause and therefore no necessary cause). On the intimate nature of the relation between cognitive and linguistic universals, also see Bever (14). The issue is of significance, for it is closely related to the question of whether (or better, in what respects), the structure of thought influences the structure of language, and it is also relevant to the matter of the evolution of language, since only "strong" and "erratic" universals can be the result of the evolution of linguistic abilities.

As Miller & McNeill point out (152), one must distinguish between universals in the sense (a) simply of a list of properties or relations among linguistic properties as empirically established, and (b) of a theory that formulates general constraints on the form of any human language. Somehow one must integrate the list of universals into the theory of grammar,

something possible only if the theory itself is "sufficiently well articulated to take account of the universals presented on the list" and if these are "stated in a form compatible with the theory." And neither condition can be met at present, for we do not know much about the conceptual status of the universals discussed by linguists such as manifest order of subject, object, and verb (85), and we therefore do not know how to link such lists of universals with what is necessarily a very schematic presentation of substantive and formal universals on the part of transformational grammarians such as Chomsky or Katz (or see some of the proposals in 6). In a sense, the problem is that of connecting incommensurables. The universals of particular concern to transformational linguists have been those of deep structure and of the relation between deep structure and surface structure, while universals of the sort discussed by Greenberg (85) reflect some aspects of surface structure, and the relation between these is at the best very unclear. Indeed, Chomsky comments (34) that "insofar as attention is restricted to surface structure the most that can be expected is the discovery of statistical tendencies such as those presented by Greenberg (1963)." Linguistic universals *qua* universals presumably reflect psychological principles of considerable generality and scope, so the inability to integrate these with linguistic theory precludes, or at least makes very difficult, any attempt at formulating such principles which, in a sense, might provide an explanation for linguistic properties. The great virtue of work such as that by Bever (14) lies precisely in the attempt to interpret a variety of linguistic and psycholinguistic findings in terms of the operation of general perceptual and cognitive principles.

LANGUAGE ACQUISITION

To begin with, let us list some articles and books which provide surveys of the relevant literature; further references will be provided below. Among books note (17, 88, 123, 186, 189). The field manual edited by Slobin (184) covers much of the significant literature in addition to providing very valuable suggestions as to how to proceed. Note also the availability in English of Jakobson's classic monograph (95). For articles which cover much of the pertinent literature see (62, 97, 152, 157, 164). For a particularly rich general account, see Braine (22), and also Slobin (188) in a paper oriented toward a presentation of recent Russian work; for a discussion with much valuable historical material, see Blumenthal (19, 20). A review of work with older children and adolescents is to be found in Griffin (87). A reading of the sources listed above, together with an examination of some basic substantive articles such as those by Brown (24, 25, 27) and Bellugi (8-11), and a consideration of some major theoretical articles (as 14, 70, 132, 177, 178, 181, 183, 192) should serve to acquaint the reader with some of the most important work and thinking in the area of language acquisition. It will become quickly obvious to any such reader that this area has been enormously

influenced by work and speculations of transformational grammarians, that the issue of nativistic determination of linguistic capacities is a crucial one, and that the most interesting and significant work has dealt with the longitudinal analysis of the acquisition of syntax on the part of a very few children, intensively studied over a span of 2 or 3 years in their lives, from age 2 or 3 up to about 4 or 5. After a look at some basic methodological issues, we shall examine some theoretical orientations toward the problems in this area, and then consider a few of the most important studies.

METHODOLOGICAL ISSUES

A very basic question is raised by Watt (200): "To what degree is the observer entitled to assume of a 'wellformed English sentence' emitted by a child that that sentence is assigned by the child's mental grammar the structure (the derivation) assigned the same sentence by the adult mental grammar?" Watt notes that both Chomsky (36) and McNeill (132) have denied the validity of such a "strong inclusion hypothesis," viz that a child's superficially well-formed sentence is underlain by the same deep structures as underlie it in the speech of an adult, and adduces both formal and natural language-based arguments in support of the denial, noting that if the hypothesis fails in the case where all of a child's utterances are "stringwise well formed, how much greater and more certain must be the failure when only *some* of the child's utterances have well-formed surfaces" (200). Given this situation, not only are there problems in interpreting much data, for example the work of Menyuk (149), who assumes that a child's sentence identical to an adult sentence must have been generated by the same rules as the adult sentence, but the general question of how to justify any particular analysis of a child's sentences in terms of the putative system governing or underlying them becomes particularly difficult. All this, of course, is exacerbated by the fact that the adult linguist is not any longer a fluent speaker of the language spoken by the child, and therefore cannot make use of his intuitions about sentences and the relations among them in arriving at a grammar.

Further, consider that, just as in the case of the adult, there is no reason to believe that a child's productions are direct undistorted reflections of his underlying mental grammar since performative factors may in various ways "refract" his output. And note that, characteristically, grammars of child language have been principally based on the analysis of a corpus of productions, a procedure regarded as generally inappropriate and insufficient when it comes to producing a grammar for adult language, but one to which we are largely constrained given that there is no access to intuitions, difficulties in questioning the child whether directly or indirectly, and the lack of "good experimental techniques for inquiring about the young child's knowledge of the structure of sentences" (27). As Huxley (123) observes, "Capturing regularities is very different from studying grammatical capacity!" and the

relation between sentences produced and the child's grammatical competence is far from direct or clear.

This immediately raises questions as to how one may assess understanding or comprehension and as to the relation between comprehension and production. As to the assessment of understanding, there is some very interesting recently published work on the response to commands by Shipley, Smith & Gleitman (180). Also Slobin & Welsh (187) in a provocative paper argue for use of "elicited imitation" as a research tool, suggesting that imitation be used as a device by which the investigator can learn about the child's language rather than "as a device by which the child can learn about the adult's language." They argue that the nature of the correct responses and errors that children make in immediate repetition may be indicative of comprehension, and thus perhaps more directly of the child's linguistic competence. Essentially this is the same procedure used by Menyuk (149 and earlier), who attempted to assess the child's grammatical competence in terms of his ability to recall immediately utterances presented by the experimenter. Studies of comprehension characteristically show, as Shipley, Smith & Gleitman (180) put it in summarizing their own research, "that children make discriminations that are not reflected in their speech . . . that a child's spontaneous utterances do not do justice to his linguistic organization." Indeed, Slobin & Welsh argue that elicited imitation can only provide a "*conservative* estimate" of the child's competence since the absence of the appropriate nonlinguistic "intention" may restrict the child's performance. Such results obviously have methodological implications tending to caution in evaluating any account of a child's linguistic knowledge based only on free production data, and they also raise substantive questions as to the extent to which and the reasons why comprehension precedes production. However, on this matter see Braine (22) for some critical, cautionary comments. As to hypotheses that might account for differences in favor of comprehension, see e.g. McNeill (132) in terms of differences in memory span, and (134) for a revised version which argues for differences in "computation" time.

One of the principal arguments in support of a nativistic account of language acquisition is in terms of the impoverished and degenerate nature of the input information available to the child—impoverished in that only surface information is manifestly available, and degenerate in that much of that is in fact ungrammatical in various ways. Nevertheless, the child rapidly, in a period of 2 years or so, picks up the essentials of language, particularly syntax. But as Fraser notes (123), it is hard to estimate quite what "rapid" means, since we cannot say with any precision when the child has mastered his language. See also Braine (22), who points out that even if most of syntax is mastered by age 4, only a fraction of the lexicon is mastered, and that this is what creates difficulties for the adult seeking to communicate with his peers. In any case, work such as that of Carol Chomsky (32), discussed later, indicates that there may be significant parts of syntax not yet mastered even by the 10-year-old. As for the ease of acquisi-

tion, Fraser (123) suggests that "for 5 or more years the child is working very hard and long hours mastering language."

Next, consider conditions of situational context and of language input to the child. What is important—the ambient speech, or what is directed to the child? Is what is directed to the child a more or less random sample from normal adult speech, or is it a specially selected or simplified version? Is it adult speech that characteristically provides the relevant input, or is it the speech of other somewhat older children? Is it speech directed at the child *per se*, or may it be that exchanges in discourse provide significant information? We are just beginning to get some information on some of these matters, particularly through the work of Slobin and his students (60) and through the work of Brown (25). Thus a pilot study by Drach (60) comparing adult-adult speech with adult-child speech found a marked difference both in the length of sentences and their syntactic complexity, with sentences addressed to the child being shorter, spoken more slowly, and syntactically less complex. And there are data mentioned in Slobin (185) that suggest that mother-to-child speech may not be strikingly different from child-to-child speech, an observation which takes on particular significance if we find, as we do, that in most societies "the major input to the language acquisition device seems to be the speech of older children" (185); this is one of the first important yields of the cross-cultural work initiated by Slobin, Ervin-Tripp, and their students. For a recent study which is methodologically significant because it does not restrict itself to the linguistic data *per se*, but seeks to infer the semantic intent of children's utterances by a consideration of the situational and behavioral context in which these utterances occur, see Bloom (17a). As to the role of the speech of other children or of the mother for the middle-class setting in our society, while it seems clear, from the findings of Brown & Hanlon (27) and others, that the speech of the child is only rarely corrected for grammatical errors, it may still very well be the case that "the changes produced in sentences as they move between persons in discourse may be the richest data for the discovery of grammar" (24). None of the above in any way permits us to dispense with innate biologically determined factors, for even in simplified form the speech directed at the young child is not and cannot be a direct spelling of deep or underlying structures. There is considerable evidence that individual children go through strikingly similar stages of development, and what little information of a cross-cultural sort is available [see e.g. Slobin's review of Russian work (188) and McNeill (133)] does support "a universality of stages and processes of acquisition." The foregoing should merely permit us to specify more precisely the nature of the input actually available to the young child, from which in some fashion he must determine or discover the grammar of the language he encounters.

THEORETICAL PERSPECTIVES

The range of theoretical positions on problems of language acquisition is defined at the one (nativistic) extreme by McNeill's formulations (132, 133) and at the other (stimulus-response learning theory) extreme by

Staats' analyses (191, 192). Theorists such as Slobin (183) and Fodor (70) take a position close to that of McNeill, but with some differences in that they both stress more a "process" than a "content" approach to the problem of universals. Theorists such as Braine (22) and Schlesinger (177, 178) are closer toward the Staats end in their attempts to specify possible learning mechanisms, but they differ from Staats both because of the seriousness with which they pay attention to and attempt to reformulate linguistic analyses, and because of their greater willingness to postulate some important nativistic, built-in determinants of acquisition.

In its essentials, McNeill's analysis is based upon and is an application the arguments of Chomsky (34) and Katz (108) to the point that "the specific content of a child's capacity for language is manifested in the form of linguistic universals" (133), and that "languages possess the universal properties contained in the theory of grammar because all languages are acquired. The renewed formulation of grammar by children automatically imposes features corresponding to the fundamental capacity for language" (152). The question is that of specifying the properties of the Language Acquisition Device (LAD=child) such that a corpus of speech passed through a device with those properties or that particular sort of internal structure will yield the appropriate grammatical system. If most universal features of language are to be found in the deep structure which is always carried into the surface structure by transformational rules, then what the child must learn is the specific transformational rules particular to its language,

making such universal features as the basic grammatical relations abstract by acquiring the particular transformations of the language to which they are exposed . . . we should expect to find that the earliest grammatical productions of the child will contain the abstract features of the deep structure . . . young children should 'talk' the deep structures directly³ (152).

In proceeding at his task the child is assumed to follow a procedure of hypothesis testing and evaluation which in many ways parallels what the linguist does in trying to construct a grammar. In an early paper (132) McNeill postulates two kinds of innate linguistic universals: (a) a hierarchy of syntactic categories, knowledge of which may direct the child to discover the classes of adult grammar; and (b) knowledge of the basic grammatical relations, viz "concepts of *subject* and *predicate* of a sentence, *main verb* and *object* of a verb phrase, and *modifier* and *head* of a noun phrase" (135). In later papers (133, 152) the first of these is given up in the face of some difficulties in interpreting acquisition data from a Japanese child (see 133), and the second proposal—that the basic grammatical relations are part of a child's capacity for language—is seen as sufficient to account for all evidence

³ If so, this would be very important in that early speech would be directly revealing of universal properties, of the essence of language.

on the hypothesis "that children sort words not according to a hierarchy of categories but according to where they occur in sentences that manifest one or another basic grammatical relation," these being used as a source of basic grammatical features, all of which are assumed to be available to the child.

The assumption that the basic grammatical relations are "honored" in the early speech of the child is supported by an examination of data from Adam, one of the children studied by Brown, where it is shown that at a time when he appears to have only three grammatical classes which could yield a total of 36 different sentence types (two or three words long) of which only 12 constitute admissible combinations, *all and only* the admissible types occur, even though adult transformed speech might well have served to provide models for some inadmissible combinations (132). The impressiveness of this demonstration is somewhat diminished, however, if one notes (see e.g. 81 with reference to 132) that examples which appear to yield inadmissible constructions such as subject-object, e.g. "Adam two-boot" are reinterpreted as meaning "Adam has two boots," and therefore become examples of legitimate combinations—thus essentially what has been shown is "that adults interpret children's speech as though it is in accord with known grammatical relations. Whether such interpretations have any objective basis is quite unclear" (81). As to the parallels between the procedure of the child and that of the linguist, questions may be raised (see e.g. Donaldson's comments in 123) as to the differences between building competence and building a model of competence and the different resources available to child and linguist respectively. Further, there are difficulties with any model in which acquisition proceeds by a hypothesis testing routine, regardless of the basis (innate or otherwise) of the hypotheses, particularly with regard to the role of negative instances, in that the absence of appropriate information about them may make it very difficult to sufficiently narrowly constrain the grammar (see 22). While largely in agreement with McNeill with regard to the importance of an innately specified component, and granting that grammatical relations and formal universals may be built in, Slobin and Fodor differ from McNeill as to the possible basis of this component. Thus Slobin (183) argues that a "child is born not with a set of linguistic categories but with some sort of process mechanism . . . a set of inference procedures . . . that he uses to process linguistic data . . . the linguistic universals then are the *result* of an innate cognitive competence rather than the content of such a competence." Fodor (70) has offered some speculations as to such possible inference or analysis procedures. Criticisms of the above sort notwithstanding, McNeill's analyses represent the boldest, richest attempt to construe the data of language acquisition in terms of a nativistic model derived from the work of generative linguists, and these formulations have served as the focus of much fruitful controversy.

Braine (22) has extended his earlier position-learning proposals for the acquisition of phrase structure, arguing that the learning of syntax is a kind of perceptual pattern learning consisting basically of a process of registering properties of the input and accumulating them in memory. The model postulates a scanner which receives input sentences and

an ordered series of intermediate memory stores the last of which is the permanent memory store which contains the rules or pattern properties that are finally learned . . . The properties of sentences that the scanner is sensitive to . . . are taken for granted . . . The scanner is 'preset' to notice certain features of the input and ignore others. Ultimately the general mathematical form of the grammars that the learner is capable of learning is in part determined by the range of properties that the scanner is sensitive to . . . Thus universal properties of natural languages are for the most part built into the acquisition model. However, the model represents a different hypothesis as to how these properties are built in.

In a sense, such a model which "can tolerate a noisy input without requiring any input information about what is not grammatical, and [which] predicts that the overall trend of learning should be from general to specific properties" might be regarded as constituting a set of "discovery procedures" for grammar acquisition—given a "preset" very powerful, richly endowed, and sensitive scanning mechanism.

Even as McNeill and other psychologists influenced by the generative approach spend much time arguing for the in-principle insufficiency of any stimulus-response (S-R) approach (see e.g. 135) so Staats, whose "complex S-R" theory (191, 192) constitutes the other polar extreme, indulges in vigorous polemic against generatively based or influenced psycholinguistic formulations. He denies the validity of most of the distinctions drawn, e.g. that between competence and performance (which he badly misunderstands and misrepresents), refuses to recognize the problems which led to distinctions such as that between deep and surface structure, and argues that no "linguistic-mentalistic" theory can ever provide any explanatory account, since independent variables cannot be properly specified or manipulated in any such theory. More, he argues that all learning approaches other than his own are limited, partial, and therefore deficient, and that what is needed is a theory of complex S-R mechanisms which encompasses the principles both of classical and instrumental conditioning (these to be taken as basic established facts) "as well as the manner in which these principles interact."

In line with this general orientation, Staats takes his task as that of indicating "the events that produce language as well as the principles by which this is accomplished" (192), and sets out to provide S-R explanations for the formation of two word utterances, the occurrence of novel utterances, the development of word classes, etc., by making use of concepts such as word association chains, response hierarchies, complex environmental stimulus control, and so on. The trouble with all this is that whenever some difficulty is encountered another schematic mechanism may be mustered to

save the day. Thus it does not help very much to say that "the variability of speech depends in part upon the constantly shifting nature of controlling stimuli" and then to argue, for example, that an embedded phrase occurs in a sentence because the speaker has just noticed a new "environmental stimulus event." Nor when it comes to a discussion of the development of the negative transformation in children is it very useful to suggest that these "observations of the child's speech could be examples of progressively learned complexity in speech. This includes the fact that new stimuli come to elicit speech responses . . . so they occur in the appropriate order and form . . . whereas other stimuli had formerly controlled the verbal responses" (192).

While the reminder that one must consider input conditions is surely a valuable and appropriate one, Staats' schematic account can provide an explanation for anything and nothing, equally well or badly. Perhaps the main difficulty is that there is an insufficient consideration of the specifics of the problems posed by the phenomena of language acquisition, together with a readiness to use schematic mechanisms or to have recourse to putative controlling stimuli for dealing with all problems that do arise. No one will be surprised to discover that Staats has been as impervious to the counter-arguments of the generatively influenced theorists as they are to his arguments—the case perfectly exemplifies talk at cross purpose, for the very phenomena to be explained or dealt with appear quite different from the one as against the other perspective.

Braine appears to be completely correct when he notes that "knowledge of phonological development has hardly increased at all during the last 10 years, and as yet there is essentially no systematic knowledge of lexical and semantic development . . . the field as a whole is in the middle of a spate of work on grammar acquisition" (22). Consequently, after brief mention of some work on semantic development, we shall look rather more fully at some examples of recent studies on the acquisition of syntax, studies of a longitudinal sort concerned particularly with the development of transformational structures.

SEMANTIC DEVELOPMENT

Very little is known about semantic development. For a discussion of some problems particularly with regard to slowness in the development of the semantic system and for a consideration of some pertinent evidence see McNeill (131). Here we shall only consider briefly some data on word associations and changes in word association, on the assumption that word associations are a consequence of linguistic competence, and that the ability to produce associations is derived from the ability to understand and produce language, and that therefore evidence about the former may be taken as providing clues regarding the latter. [See also (44) on the psycholinguistic relevance of word association data.] Associative responses may be

classified as paradigmatic (same syntactic category as stimulus) or syntagmatic (different syntactic category). There is much evidence that young children are prone to give far more syntagmatic responses than adults, and that between 5 and 9 years children go through a "syntagmatic-paradigmatic shift," changing from giving mainly syntagmatic to mainly paradigmatic responses. Assuming that in the word association task the subject is trying to produce a word that is similar or related to the stimulus word, and that he does this by attempting to match features in the stimulus with features in the response, McNeill (131) and subsequently Clifton (48) argue that many of the young child's syntagmatic responses may really be "pseudosyntagmatic," reflecting principally the paucity of his semantic feature system, such that minimal feature contrasts may involve contrasts on syntactic category features rather than semantic features as appears to be the case for adults. Thus the "syntagmatic-paradigmatic shift" may not involve so much a change in processing of information as a change in the information on which the same process operates, not an increase in the tendency to match semantic features so much as an increase in the number of features available for matching.

Consistent with the interpretation that child-adult differences reflect a lack of appropriate semantic constraints in the child's associations, an examination of young children's syntagmatic responses reveals that these are characteristically different from adult syntagmatic associations in that they often violate grammatical selection restrictions (by standards of the adult system). Thus we have not only a "syntagmatic-paradigmatic shift" but also a syntagmatic shift, i.e. a total shift in the nature of the associations given, in the direction of greater semantic consistency and control. Evidence from young children's response to anomalous sentences, their inability to take advantage of semantic consistency in the recall of sentences, argues to the same point of semantic immaturity. As McNeill puts it, "children show a readiness to produce combinations of words that violate some selection restrictions, which perhaps means that the semantic markers that figure in the violated selection restrictions are not yet part of the children's dictionary" (131). Considering the "syntagmatic-paradigmatic shift" as symptomatic of the acquisition of an adult lexically coded memory system, Anderson & Beh (3) conducted some experiments in short-term recognition memory for familiar words, obtaining supporting evidence for McNeill's view that this shift is indicative of the development of a system of semantic markers which serves as an efficient memory code.

SYNTACTIC DEVELOPMENT

We turn now to a consideration of some recent very important studies on the acquisition of syntactic structures by young children, looking first at some work by Roger Brown and his associates (8-11, 24, 25, 27, 110), and finally at a study by Carol Chomsky (32) concerned with older children.

The data on which these studies are based consist principally of the spontaneous speech of some three very young children and their mothers in conversation at home, supplemented by a number of quasi-experimental procedures [see e.g. the use of the "puppet-game" to elicit *wh*- questions in (10)], and on occasion by experimental studies using a larger number of children [see e.g. Cazden's dissertation research (30) on the role of expansions in grammatical development]. The longitudinal data for each child extend over a period of the order of 1 or 2 years (from about 1½ years to 4 years), with developmental stage defined not chronologically but in terms of mean utterance length. Brown and Bellugi and their co-workers provide an extraordinarily rich amount of their findings, at a level which is both very responsive to the specifics of the data and at the same time attempts to indicate some general properties which put these in theoretical perspective. As Brown notes (25), the research is directed at two general questions: "What does the child know of the structure of English at successive points in his development? By what process does he acquire his knowledge?"

In Brown, Cazden & Bellugi-Klima (25) there are presented portions of a sample grammar, written for Adam at Stage III, which gives perhaps the best representation available of the nature of such a child grammar written from the point of view of a generative analysis, providing a discussion of phrase structure rules, subcategorization rules, and transformational rules. It is instructive to compare this sketch of part of a grammar published in 1968 with an earlier account by Brown & Fraser (26) first published in 1963—the more recent paper is vastly richer and theoretically very much more sophisticated.

The development of interrogative structures is discussed at length in Bellugi (8) and in Klima & Bellugi (110); particular attention is given to the development of *wh*- questions in Brown (24). An account of the development of negation is also presented in (110); see also McNeill & McNeill (138) especially on the development of the semantics of negation with particular reference to Japanese. There is also a consideration of segmentation into morphemes (25) and some suggestions as to how this might be accomplished as efficiently as it generally appears to be accomplished by the child. There is a discussion of the development of self reference, of conjoined pronouns and case marking in children's grammars (9, 10) in terms of "ever increasing conditions on the applicability of rules" (10). Some data on the acquisition of noun and verb inflections may be found in Cazden (31).

These studies provide perhaps the fullest accounts available with regard to the ways in which interrogation and negation are expressed initially, and on the development of transformational systems which appear to be very similar for all (three) children. The results can hardly be characterized in a brief summary, so only two matters will be noted here: (a) The widespread appearance of novel grammatical features (at Stage III) when auxiliary verbs appear simultaneously in declarative sentences, negative sentences,

yes/no questions, and sometimes in *wh*- questions, as well as the appearance of embeddings at about this time, suggests that the general concept of transformational rule has now become available to the child. (b) There appeared to be a time when the children had mastery of structures that required a number of different operations, but not if these were called for simultaneously—thus the child might be able to “perform the operations necessary for yes/no questions and *wh*- questions, but not when auxiliary inversion and *wh*- preposing are both required” (9), or inversion might appear in “*wh*-word questions which were in the affirmative but not in the negative *wh*-word questions,” as though there were a performance limit on the number of operations that the child might simultaneously perform on a simple string. Note that in speaking about operations here we are co-ordinating the presence of additional grammatical rules with greater complexity in processing, an assumption that has its hazards.

One may be concerned with the use and replacement of “immature” or transient structures in child language (10), or one may examine the “determinants of the order of emergence of mature structures in a family of related grammatical structures” (27). Focusing on the development of questions, negation and case marking, Bellugi-Klima (10) finds immature but consistent structures which are syntactically deviant in regular ways from the language the child hears, as if sentences were constructed according to a different set of rules, with systematic deviations from adult language representing certain types of syntactic simplifications. For example, with regard to the relation between negative and indefinite forms it is as though first the dependency relation between them is ignored, then there is a simplification to one generally applicable rule (in place of the complex set of interdependent rules in adult English), and finally something corresponding to the adult system emerges, with earlier forms perhaps representing limits on the permitted complexity of sentences in the child’s grammar (and therefore impervious to attempts at correction by adults). On the basis of these systematic differences and general changes in child language, Bellugi-Klima argues that what the child is doing is to extract regularities from the speech he hears, reconstituting or reconstructing a version of the complex underlying system of rules involved, that “the child imposes an *internal* ordering on the linguistic data he is exposed to” (11), and that the “language system develops in the child by ever-increasing (and sometimes over-increasing) conditions on the applicability of rules” (10).

Brown & Hanlon (27) sought to determine whether derivational complexity is related to the order in which mature constructions emerge in child speech, limiting themselves to “cumulative” relations of derivational complexity, i.e. instances where the derivation of one sentence involves everything required in the derivation of another, plus some additional rule or rules. Very much aware of difficulties in supposing any simple or direct relation between derivational complexity and difficulties in processing or psychological complexity, they argue, nevertheless, that

the child's production of a construction in quantity and over a good part of its proper range, together with evidence that he understands the construction when others use it [may be treated] as *signs* that he has the grammatical knowledge represented formally by the derivation of that construction in adult grammar . . . [and] that the order of emergence of such knowledge would reflect derivational complexity even when the *speed* and *accuracy* with which such knowledge is employed in understanding and retaining sentences does not . . . since a grammar formalizes adult knowledge it is reasonable to hypothesize that the child's knowledge of the structure of his language grows from that which is, in the grammar, derivationally less complex to that which is derivationally more complex (27).

Brown & Hanlon consider the emergence of eight different sentence types in child speech ranging from simple, active, affirmative, declarative (SAAD), or interrogative (Q) sentences at the one extreme to simple, active, negative, declarative, truncated (TrN) or interrogative, truncated (TrNQ) sentences at the other extreme of derivational complexity. In terms of a grammatical analysis which yields a partial ordering among the sentence types it is possible to make 19 predictions as to their order of emergence. Using a necessarily arbitrary but nevertheless plausible criterion to define acquisition, they find strong support for the hypothesis that order of emergence in production reflects derivational complexity in terms of the adult grammar. For the three children considered, 16, 15, and 16 of the 19 predictions respectively are supported, and in only 2, 2, and 0 cases does the evidence go the other way. The order of emergence is SAAD, Q, N, Tr, TrN, TrQ, and TrNQ (NQ does not appear earlier than TrNQ). The evidence indicates that with regard to differences in cumulative derivational complexity "new rule" complexity is more clearly a determinant of the order in which knowledge is manifested than is "added rule" complexity, as though coping with an expression involving something completely new is more difficult than coping with one which involves a number of structural properties each of which individually is already under control. Bever (14) presents an alternative view with regard to these findings, suggesting that "the data could be explained equally well by a view of sentence complexity according to which the more internal structure material implicit in the external structure the harder the sentence, since the child must contribute more information to the sentence himself."

While sentence length and derivational complexity are partially confounded, it does not seem likely that the former variable could account for these results since for those cases where they entail different predictions the predictions based on derivational complexity are far more successful than those based on sentence length (the box score is 31:1). Although frequency in parental speech and derivational complexity are confounded, Brown & Hanlon do not believe that the frequency variable can account for order of emergence in child production. Instead they suggest that a form produced with high frequency by parents will "be somehow represented in the child's performance even if its structure is far beyond him . . . the construction will

become lodged in his speech as an unassimilated fragment" (27). On this point, while very likely correct, their arguments and evidence are not completely convincing [on this and related matters see Watt's comments in (88)]. There is evidence that expansion of a child's utterance does not necessarily aid in the acquisition of grammar (30); but the interpretation of these findings is not clear [for three possible suggestions see (25)].

Given that even at an early stage the child is able to make statements, ask questions, negate, and make request for confirmation, it does not seem too plausible that the basic semantics underlying these account for the order of emergence of mature grammatical structures. Nor does it seem likely that ill-formed constructions in child speech give way to well-formed ones because of a selection pressure in communication which favors the latter, since adult interlocutors appear generally to be able to understand the ill-formed expressions about as well as the well-formed ones, and do not appear to respond differentially to the two kinds of expressions. Nor is there any evidence to support the argument that syntactically well-formed expressions are selectively reinforced by parents or ill-formed expressions selectively punished. An examination of the data reveals that it is "truth value rather than syntactic well-formedness that chiefly govern verbal reinforcement by parents—which renders mildly paradoxical the fact that the usual product of such a training schedule is an adult whose speech is highly grammatical but not notably truthful" (25). In any case, as noted above, children seem remarkably impervious to attempts directly to correct their grammar. Brown & Hanlon end by suggesting that "the only force toward grammaticality operating on the child is the occasional mismatch between his theory of the structure of the language and the data he receives." They further suggest that in this regard recurrent discourse patterns involving sentence and constituent exchanges which are rich in structural information may be particularly helpful to the child [see particularly Brown's discussion on the possible role of "occasional" questions in (24)]. Thus, "the structural information latent in various forms of linguistic interaction may be of crucial importance," and "the changes sentences undergo as they shuttle between persons in conversation are . . . the data that most clearly expose the underlying structure of language" (25).

The general suggestion offered by McNeill as to why child language changes (152) is that "the driving force behind language development is the rapidly growing variety of semantic interpretations for which the child must find some means of differentiation and expression," all this happening presumably as the child's cognitive and information processing capacities are developing and maturing. As McNeill notes, "to say that a need for precision and cognitive economy motivates the developmental sequence is not to explain how a child is able to meet these needs" in the particular manner he does. McNeill's answer is to call on the "child's innate *faculté de langage*" which permits him to formulate hypotheses concerning the expression of some semantic content, the validity of which may be tested

against parental speech, which thus serves selectively to guide invention. A rather different account of the development of perceptual strategies relevant to the processing of speech, emphasizing the unity of cognitive processes and of the psychological system underlying perception and memory may be found in Bever (14).

Thus far we have considered syntactic development in the child under 5. But what about further and later developments? Here we illustrate some possible ways of proceeding and some findings by reference to Carol Chomsky's monograph, *The acquisition of syntax in children from 5 to 10* (32). Chomsky sought to test the child's knowledge of complex syntactic structures by investigating his ability to interpret correctly sentences exhibiting these structures, under conditions where there were no contextual or semantic clues that might influence interpretation. She elicited information from children by direct interviewing involving the playing of games, carrying out of tasks, and answering of questions, seeking to determine whether the child not only had semantic knowledge of the word, knew "the concept attached to the word," but also knew the constructions in to which the word could enter. In order to understand a sentence a listener must be able to determine the grammatical relations which hold among the words and phrases that constitute it, and Chomsky argues that this task is made more difficult, the sentence becomes more complex, insofar as

(a) the true grammatical relations which hold among words in a sentence are not expressed directly in its surface structure; (b) the syntactic structure associated with a particular word is at variance with a general pattern in the language; (c) a conflict exists between two of the potential syntactic structures associated with a particular verb; and (d) restrictions on a grammatical operation apply under certain limited conditions only . . . [Thus] structures which have potential for late acquisition are ones which deviate from widely established patterns in the language or whose surface structure is relatively unexplicit with respect to grammatical relationships (32).

She found usable examples satisfying each of the above conditions and investigated their acquisition in children from ages 5-10 years. To mention but two (related) examples, it is characteristically the case in English that for sentences of the form "NP₁ V NP₂ to infinitive verb," NP₂ is assigned as subject of the infinitive verb (as in "John told Bill to leave"), and this has been formulated as the Minimal Distance Principle (MDP), viz that "the implicit subject of the complement verb is the NP most closely preceding it." However, there are cases where this does not hold, e.g. the verb PROMISE ("John promised Bill to go"). There are also cases where it holds under some circumstances and not under others, e.g. ASK where the Minimal Distance Principle holds for (a) "ask+to+infinitive verb" (e.g. "John asked Bill to leave"), but not for (b) "ask+wh- to+infinitive verb" (e.g. "John asked Bill what to do"). Presumably one should expect the child to have difficulties with PROMISE and even more difficulties with ASK, and this is exactly what was found. While the structure involving PROMISE

is acquired between 5½ and 9, and known by all children 9 or over, the structure involving ASK "is still imperfectly learned by some children even at age 10." On the basis of this finding plus the results of some informal questioning with adults, Chomsky suggests that this may be a syntactic structure that is never properly learned by a substantial number of adult speakers [cf. some of the paraphrase results of the Gleitmans for noun compounds below (81)]. This raises some provocative questions with regard to the nature of adult syntactic competence. In general, Chomsky's results indicate that some syntactic structures may indeed be acquired quite late, and that the relative order in which the structures studied are acquired is constant from child to child. The study yielded substantive results of considerable interest (only a few of which have been mentioned), and methodologically may serve as a model for how to proceed in future studies of the mastery of complex syntactic structures.

EXPERIMENTAL PSYCHOLINGUISTICS

Such is the proliferation of studies in experimental psycholinguistics that our treatment must be even more highly, not to say brutally, selective, inevitably ignoring much work of importance. After some introductory comments we shall consider work relevant to phonology, syntax, and semantics, with particular emphasis on syntax, since conceptually this is central (from the perspective of the transformationalist it constitutes the generative component of the grammar), and since this has been a particularly popular problem area for psycholinguists. For a paper which constitutes a particularly rich review of data in speech perception as well as offering a theoretical program for their interpretation, see Liberman et al (120). An overview of much recent work on the perception and memory for sentences may be found in Bever (13), in a paper valuable particularly for the suggestions it offers with regard to possible processing strategies or mechanisms; see also a survey paper by Garrett & Fodor (79), and an attempt to characterize "the organization of linguistic performance" by Wales & Marshall (197).

The view of language which has been focal in recent research, perhaps just because contemporary psycholinguistics has been so strongly influenced by work in generative grammar, is one of language as idea, knowledge, or mental structure, largely or entirely abstracted from its setting, from problems of communication particularly in, say, the context of dialogue, and without much serious worry about normal temporal constraints. Obviously such a view of language concerned principally with the perception of and memory for short stretches of monologue excised from any setting and without continuity, to caricature a little (but only a little) is far from being the whole story, and there have been protests, perhaps most vigorously by Rommetveit (see 166, 167).

In carrying out research, psycholinguists have employed the standard repertoire of perceptual, cognitive, learning, and memorial procedures, and

have also devised all sorts of novel techniques. One general sort of problem arises with regard to much of this work—it is often not clear whether it is primarily directed toward the assessment of the psychological reality of some syntactic structure or, say, toward an analysis of how a sentence might be learned or remembered. Obviously these are not independent issues, nevertheless it seems clear that inferences about underlying syntactic structures cannot be made directly in terms of errors or confusions on a memorial task. Such inferences must be called into question because the sort of syntactic analysis required in a memory task may differ in significant respects from that required for full understanding, and, more important, because such data provide information simultaneously about possible underlying structures and about memorial and retrieval processes, and generally it will not be possible to factor this information into its components (see Fillenbaum 65a). Short-term and longer-term memory and learning tasks have often been used as indirect indices of comprehension, just because it is very difficult to contrive good direct procedures. This argues for caution in interpreting the results of such studies. In any case, we should be alert to the possibility that the results of some (perhaps much) experimental work may be as much a consequence of special ad hoc strategies adopted for coping with particular laboratory tasks as of anything else, and that often subjects may not at all be dealing with what we want them to deal with, and what we think they are dealing with. To take but one example, if all sentences in a learning and recall task are of the surface form "They are verb-ing N-pl," subjects may be treating such strings not as sentences but as nominalizations involving just the last two words, leading to quite different predictions from those that would be made if full sentences were being processed [see e.g. (165) where the author was sensitive to this possibility].

With regard to theoretical perspectives guiding experimental work, it appears clear that the major impetus has come from analyses due to transformational grammar. The only other linguistic account that has had any impact on experimental work is that of Yngve (208), whose proposals with regard to phrase or surface structure as embodied in the "depth" hypothesis have been taken as the basis for a psycholinguistic model by E. Martin and his associates (142, 144), and have also figured in the work of Johnson (101, 102). In some major respects Yngve's formulation would appear to be implausible as a psycholinguistic model, for it requires that performance proceeds from the top down as though a speaker first decides on the structure of a sentence before choosing what he is going to say, and as though a "hearer does not begin to process a sentence until after he hears it in its entirety" (159), as well as entailing an asymmetry between the model for the speaker and that for the hearer or listener. In addition, it should be noted that while Yngve stresses that the *maximum* allowable depth of a sentence is constrained by memory limitations, experimenters have always used *mean* phrase structure depth (average number of left branches per word) as a measure of structural complexity, and it is not at all clear that

this is an appropriate embodiment of Yngve's model. As to experiments using the mean depth measure, the results are mixed at best. Thus Martin & Roberts (142) found that differences in mean depth were related to difference in free recall of sentences, and Martin, Roberts & Collins (144) obtained supporting data using a short term memory task; however, Martin & Roberts (143) found recall unrelated to mean depth. Perfetti (158, 159) found no evidence in support of the depth hypothesis, and Wright (207) has mixed evidence at best. One may consult Rohrman (165) for a general analysis of difficulties with the depth hypothesis, particularly as used by Martin & Roberts, for difficulties with their attempt to equate sentences in transformational terms, and most important perhaps, for a demonstration that, for materials with identical surface structures but of differing deep structure complexity, differences in recall are predictable from the latter—a result incompatible with any model, such as Yngve's, which is phrased purely in terms of surface structure.

One other controversy of very general importance is that between Fodor (68, 69) and Osgood (155, 156). Fodor argues for the in-principle-insufficiency of any representational mediation theory as the basis for a theory of meaning and for the identity in crucial aspects between two-stage S-R models and one-stage models. Osgood defends his views by stressing particularly "the functional separation of decoding and encoding phases" in his theory, that representational mediators are assumed to be *componential* in character, and the fact that "the distinctive semantic features (components of r_m) established for primary perceptual and linguistic signs are capable of complex, novel and indeed 'emergent' interactions" (156).

Finally, one should note two papers by MacCorquodale (124, 125), the first of which is a retrospective appreciation of Skinner's magnum opus *Verbal Behavior*, and the second of which constitutes an attempt to rebut Chomsky's classic review of that book—both papers deliberately written as though nothing much had happened in psycholinguistics in the last decade or so.

SPEECH PERCEPTION AND PHONOLOGY

The major event is the publication of a paper by Liberman et al (120) which reviews a large body of data on speech perception, arguing "that the sounds of speech are a special and especially efficient code on the phonemic structure of language, not a cipher or alphabet" and that the central fact of speech perception is "a marked lack of correspondence between sound and perceived phonemes." Noting "the restructuring of phonemes at the level of sound" such that the same acoustic input may be heard quite differently and different inputs can give rise to the same phoneme as a function of the acoustic environment, it is argued that many phonemes (particularly consonants) are encoded so "that a single acoustic cue carries information in parallel about successive phonemic segments," that there must be a special speech mode (on this point see particularly 147), that "the relation between

phone and sound is that of a complex and efficient code bearing a formal resemblance to the codes we know as syntax and phonology," and that the relation between articulatory parameters and perceived speech is much tighter than that between acoustic parameters and perceived speech. This leads to a model of speech perception where phonemes are identified by referring incoming speech sounds to neuromotor commands to the articulators. [See also (121) for "some observations on a model for speech perception."] For some objections to a motor theory see Lane (114), and for a reply to these criticisms see (194a); for an alternative approach in terms of a theory of context-sensitive coding see Wickelgren (206).

Wickelgren provides evidence (203) that a verbal item may be coded in short-term memory as a set of phonemes, each of which may be forgotten independently. The obvious next question is to ask whether each phoneme may itself be coded as a set of distinctive features, each of which may be forgotten independently, and indeed there is evidence (204) that a vowel is coded in short-term memory not as a unit but in terms of two distinctive features each of which may be forgotten independently, and that consonants also may be coded as sets of distinctive features each of which may be forgotten at least semi-independently (205). There is work too that suggests that phonological distinctive features may serve as cues in learning, and that articulatory cues may be particularly important in this regard, since subjects instructed to "pay attention to what your mouth is doing" performed especially well (99).

One may ask further about "the role of distinctive features in the psychological functioning of linguistic *rules*." Anisfeld and his associates (4, 5) have attempted to do just this, using a task where the subject is required to select which of two synthetic words differing only in a final consonant would make the best plural, or best past tense verb, or best derived adjective (given systematic differences in the last syllable). With regard to the pluralization task, most of the sounds preferred by adults and children as artificial plurals "share with the regular plural suffix /s z/ the phonological features +strident and +continuant which are more closely identified with the plural than are any other features," i.e. the features most specifically relevant to the differentiation of the plural suffix (5). For the case of adjectives derived from verbs, Anisfeld bases his analysis on a recent generative phonological formulation due to Chomsky & Halle (39), seeking to test the psychological validity of the postulated abstract high-level phonological rules (or at least of their products). In terms of this formulation in the derivation of /iv/ adjectives from /d/-final verbs there is an intermediate stage, while there is no such intermediate stage in the derivation of adjectives from /t/-final verbs. Thus, *permit* is converted directly to *permissive*, while *extend* is changed first to *extensive* and then by the application of an additional rule to *extensive*. Considering artificially constructed verbal and adjectival forms, presumably this implies that *dalpondzive* should be more acceptable as the adjective corresponding to the verb *dalpond* than *yermizive* as the adjective corresponding

to the verb *permit*. Indeed while /z/ was the most preferred adjectival form both for verbs ending in /d/ and /t/, it was significantly more highly preferred in the former case (4). These and other data presented by Anisfeld suggest that the functional rule-mediated relations among sounds, rather than absolute similarities, determine judgment. Obviously results of this sort raise, at the phonological level, some issues with regard to the psychological reality of linguistic rules and structures which are very similar to issues in the domain of syntax, e.g. what, if anything, may be taken psychologically to represent or correspond to the cyclic application of rules which figures so prominently in the theory of generative phonology.

SYNTAX

If a grammar is based on intuitions regarding sentencehood and the structural relations among sentences, then it would appear appropriate for the psycholinguist to concern himself with matters such as the grammaticality (or perhaps better "acceptability" in the sense of Chomsky, 34) of sentences, paraphrase relations, sentence ambiguity, and inter-sentence relations. We shall consider some work relevant to each of these issues, and in the course of the presentation will deal both implicitly and explicitly with data relevant to the distinction between surface and deep structure, and will say something as to how sentences may be perceived, understood, and remembered.

Grammaticality.—Coleman (50) constructed series of words at four levels of grammaticalness, ascending levels being defined by putting successively more severe restrictions on the arrangement of word classes. Subjects ranked the word sets for grammaticality as predicted, and learning of the sentences was related to level of grammaticality. Marks (140) had subjects rank according to their grammaticalness normal sentences and semigrammatical sentences derived from the normal sentences by inversion of a pair of adjacent words, or of a word and an adjacent phrase. He found that, in general, semisentences in which the inversion of word order disrupted boundaries of major phrases were judged less grammatical than those cases which left phrase boundaries intact, and that the earlier in the string the inversion occurred, the less grammatical the string was judged to be; reaction time results in a sentence identification experiment were quite parallel (139). Danks (53) attempted to study the comprehension process more directly by obtaining ratings of comprehensibility and examining latencies to understanding of sentences, and found that meaningfulness and grammaticalness were the principal variables affecting comprehension. Downey & Hakes (59) approached the study of acceptability in a more analytic fashion by constructing sentences embodying three types of rule violations varying in generality from (a) violation of phrase-structure rules, through (b) violation of strict subcategorization rules, to (c) violation of selection restrictions—these types of rule violation being strictly ordered—and had subjects rate

these sentences, learn them in a free-recall task and paraphrase them. Sentences were ordered as predicted on the rating task, but in free recall the effects of selection restriction and strict subcategorization rule violations were the reverse of those predicted.

While results of the sort reported above suggest that something fairly orderly is being tapped by judgments of grammatical acceptability, and that differences in this regard may be related to differences in other performances, they do not indicate clearly what is being tapped. Rather, they reveal that such acceptability judgments are sensitive to or are contaminated by aspects of utterances not generally thought to be matters of grammar, as e.g. Marks' finding that the earlier in a string an inversion of word order occurs the less grammatical it is judged to be, or the sorts of data that indicate that differences in meaningfulness may affect subjects' estimates of grammaticality. "In short, the ability to classify by grammaticality . . . long considered the most direct and crucial empirical correlate of transformational grammar . . . has not as yet been demonstrated" (81) because we do not know how to instruct the subject appropriately so as to get at his intuitions about grammaticality uncontaminated by other factors, because judgments of acceptability cannot be taken as directly and simply revealing regarding such intuitions. (For some critical comments on the distinction between grammaticality and acceptability see 162.)

Paraphrase.—If it is the "interweave among constructions that lies at the heart of the transformational hypothesis" (81), then a display of paraphrasing abilities should be the most natural way to show the adequacy of such a grammar. It is this argument which motivated a very rich study by the Gleitmans on the paraphrasing of compound nouns. They selected compound nouns as their materials since noun compounding is a central syntactic device in English, and can be described "only by reference to the kinds of recursive processes that provide the basis for novel syntactic behavior" (81). This material also has the virtue that complexity and internal structure can be varied while using short phrases, all of whose individual words are common, well known items of vocabulary. In American English there is a special paraphrastic relation between compound nouns and relative clauses such that the "*compounding transformations uniformly have the effect of reversing the serial order of elements in the relative clause*" (81), plus specifying some special compounding intonation. Thus the source for compound nouns is a noun phrase containing a relative or a preposition, the recursive structure of the base rules being such that the corresponding rule can be applied to its own output. To the extent that people can devise just those paraphrases isolated by the syntactic description, showing their command of the rules of compounding by decoding and recoding novel compounds, there is evidence regarding the availability or psychological reality of that critical part of the transformational grammar. It should be noted that while characteristically intimately related, understanding and paraphrase are not equivalent since

the former is neither a necessary nor a sufficient condition for the latter, and that if people can provide paraphrases in a principled way even for expressions deviant in various ways, then "something very like a transformational syntactic organization must be postulated to account for this skill."

The research was designed so as to confront subjects with novel, short, three-word items (composed of familiar words) involving two applications of the compounding rule, and required subjects either to provide a paraphrase, or choose which of two alternatives constituted the more appropriate paraphrase. Each three word compound had two "constant" items (*bird* and *house*) plus one other item which might be a verb, adjective, or noun (or have more than one classification). Order of constant items, position of variable items, and stress were systematically manipulated. Concerned with what they consider an unrealistic "democratic" assumption which attributes equal competence to all speakers, the Gleitmans selected subjects at three educational levels: (A) graduate students and PhDs, (B) undergraduates or graduates who did not intend to continue on with graduate work, and (C) secretaries with high school diplomas and no intention of continuing on to college. The results of the paraphrase production and selection tasks were similar. While none of the subjects had any trouble understanding instructions, there were "massive" differences between population groups in the ability to provide syntactically determined paraphrases. The less educated groups made far more errors (mean of 62% for C versus 45% for B and 13% for A on the paraphrase production task), and to a significant extent, different errors than the most educated group. All groups were affected similarly by certain syntactic features of the compounds, e.g. phrases containing variable terms that were verbs or postnominal adjectives were much more difficult than those involving nouns or prenominal adjectives, and semantic plausibility interacted significantly with error frequency and type. While subjects often did err, the fact that they were as successful as they were in their paraphrases is of great significance vis à vis a transformational account; as the Gleitmans put it, "if the reader does not find this surprising, let him consider whether any other theoretical description would allow us to predict in any precise way what form paraphrases would take." They interpret their results as requiring a distinction between a core grammar of great range and systematicity whose rules reflect very pervasive regularities of the language, and a penumbral grammar concerned with quasi-systematic relations or restricted and partial regularities, with people all in command of the core grammar but differing in their knowledge of the penumbral grammar, and in the skills required for paraphrasing sentences which are semigrammatical (on a person's knowledge of the grammar).

In many ways this study can serve as a model for future investigations, using paraphrasing techniques to investigate other aspects of grammatical knowledge. But on one basic point, while in some measure persuasive, it does not appear completely convincing: can one really be confident that tasks of the sort employed actually directly tap "competence" and there-

fore reveal differences in competence as between different subject groups? What if subjects had been confronted with a task whose items involved only one application of noun-compounding rules, and if everyone had performed about equally, and with very few errors? Would one then still be willing to regard the results obtained in the present study as revealing of differences in knowledge of the grammar, or would one be more ready to interpret them as revealing of differences in the ability to apply such rules recursively? What if the task had involved compound nouns requiring four or five recursive applications of the rules, with everyone getting almost all items wrong? Would one be willing to take this as evidence for the lack of grammatical knowledge, rather than of the inability to reapply it as many times as needed? Certainly the Gleitmans would have a firmer basis for their conclusions if they had also obtained similar results with a task requiring only a single application of the compound noun rule. Very similar questions arise with regard to data such as those of Stolz (194), who found that a substantial proportion of Harvard students could not understand multiple self-embedded sentences (given all sorts of aids), even though everyone could cope with sentences involving a single self-embedding. It is not clear whether the issue is an empirical one or whether what is involved is a conceptual or definitional matter: given that people have command of a rule which may be recursively applied (without limit) in the grammar, but which their performance reveals they can apply only once, or twice (or whatever), shall one say that they really know the rule but that there are performance limits on its iterated application, or shall one say that they do not really have command of the rule since, *qua* rule, there are no limits on its recursive application.

Ambiguity.—Sentences which are ambiguous are of interest to the linguist because of the constraints they impose on any grammatical description, e.g. there are certain sorts of ambiguities that cannot be readily characterized unless one makes a distinction between surface and deep structure. Ambiguous sentences are of interest to the psychologist for their own sake as he seeks to determine how an ambiguity may be detected by the listener and whether different sorts of ambiguities are harder or easier to detect, and for the implications such results may have for the normal processing of unambiguous sentences (and for an understanding of the fact that characteristically people do not notice the ambiguities of ordinary discourse). Here we shall only note that both in a task where the subject was required to perceive the two meanings of each of a series of ambiguous sentences (128), and in a task where he was required to complete each of a series of ambiguous sentence fragments (126), it was found that performance was better for items involving lexical ambiguity than for items involving surface structure ambiguity. Performance on the latter sorts of items was better, in turn, than performance on items involving deep structure ambiguity, and, other things equal, performance was better for items involving a single ambiguity than

for items involving multiple ambiguities. As to the nature of possible processing mechanisms, Foss, Bever & Silver (76) consider the results of a sentence verification task which showed more errors and longer latencies in response to the unexpected alternatives of ambiguous sentences than to the expected alternatives (with no difference between the latter and control unambiguous sentences). From this they reach a model of normal sentence comprehension according to which only one meaning of an ambiguous sentence is perceived at first. Taking issue with this position which he calls the "non-interaction" hypothesis, MacKay (127) argues for a "perceptual suppression" hypothesis which requires "interaction between perceptual analyzers in the comprehension of speech" with parallel processing. He bases his analysis on the finding that the time to perceive ambiguity decreases with the salience of one of the alternatives, and that the time to complete ambiguous sentences increases with salience when the more probable meaning is seen, and decreases when the less probable meaning is seen. The principal difficulty is in deciding what inferences may be made with regard to normal sentence processing from data on a task where the subject is *explicitly instructed in advance* that each sentence is ambiguous and required to find the ambiguity as quickly as possible (128), or from sentence completions of (ambiguous) fragments where median completion times run over 7 seconds at the fastest (126). This raises serious doubts as to whether normal mechanisms of immediate sentence understanding are being tapped.

Intersentence relations and the coding of grammatical features.—Clifton & Odom (49), concerned with the syntactic relationships among sentences, investigated the "similarity relations among certain English sentence constructions" viz. simple active, affirmative, declarative (SAAD), passive (P), negative (N), question (Q), PN,PQ,NQ and PNQ sentences. They dealt principally with the confusions among the eight sentences of a P,N,Q sentence family in a recognition task, analyzing the data by means of metric and nonmetric multidimensional scaling techniques. "Over all studies much the same relationships were obtained." The data could be represented very adequately by a prism whose base is defined by SAAD,P,N, and PN all substantially separated, and whose top is defined by Q and NQ, and PQ and PNQ, with the members of each of these pairs very close together, and the pairs themselves midway between the affirmative and negative nonquestions. The constructions fall into a similarity pattern very consistent with that implied by a grammatical analysis due to Katz & Postal (109), in which differences between the constructions are introduced by means of phrase structure rules in the base component, rather than via transformational rules. Thus such results support the hypothesis that sentences may actually be held in mind in a way consistent with a particular grammatical account of their structure [but note our earlier caution with regard to the interpretation of memorial data and see some pertinent findings in (65a)].

Savin & Perchonock (174) were directly concerned with the way in

which sentences might be coded and stored in memory. Assuming that immediate memory has a small fixed capacity, and that sentences are stored in terms of their underlying structure, they suggested that the amount of memory space needed for a sentence could be measured by noting how much irrelevant material could be remembered along with it, and that by employing sentences of different structure one could thus determine the amount of immediate memory occupied by various grammatical features such as negative, passive, question, negative-question, emphatic and *wh*-transformations (on the assumption that each of these features is encoded separately and occupies a characteristic amount of space in immediate memory). The subject was always given a sentence followed by a list of eight unrelated words, and was required to recall first the sentence and then as many of the words as he could. The results were very clear-cut: not only were fewer words remembered for sentences requiring more transformations, but the data had strong interval properties permitting one to make consistent estimates of the amount of memory space taken up by each of the transformations. Unfortunately, it is not at all clear whether these results hold up, or whether they require the sort of interpretation offered by Savin & Perchonock. Matthews (146) replicated their study but did not find any effect of transformational complexity upon recall of unrelated words. Glucksberg & Danks (82) found only weak effects of transformational complexity on word recall, and argued that these effects might well be attributed to differential word delay since the more complex the sentence the longer it tends to take to say, and therefore the longer the delay between word presentation and word recall (word recall was found to be significantly related to word-recall delay). Finally, Epstein (61) replicated the Savin & Perchonock study using two recall conditions—"sentence then words" or "words then sentence," with recall cue following the last word. With words recalled second, as in the earlier study, Epstein obtained roughly the same results. However, when words were recalled first these effects were almost completely eliminated, which suggests that differences in word recall may perhaps be due to interference from prior recall of the sentences, but in any case tells strongly against the original Savin & Perchonock formulation which must surely require that there be no systematic differences in the results as a function of order of recall of sentence and words, since it is a hypothesis regarding the initial coding and storing of the sentence materials.

Above we have touched on some studies regarding the way in which sentences of different structure might be coded. Now we should mention work which attempts to say something about the circumstances governing the employment of syntactically different but semantically largely equivalent structures, viz sentences in the active and passive voice. Johnson-Laird had subjects draw diagrams to illustrate an active sentence and its corresponding passive (104), and also had subjects choose the most appropriate description for a diagram from a set of active and passive sentences (103) on the hypothesis that the passive is used to emphasize the logical object. He obtained

evidence in support of the emphasis hypothesis (as well as for a word-order factor in the latter study). The paper "What is so difficult about negation?" by Wales & Grieve (196) attempts to provide some account of the factors which may be involved in difficulties with use of a particular kind of construction. In both of these instances one is dealing with some pragmatic properties or consequences of the use of a particular syntactic construction.

Surface and deep structure.—A distinction between surface and deep structure has figured in much of the preceding presentation. We shall now present some evidence on the cognitive reality of (some properties of) surface and deep structure. With regard to surface structure there is much evidence from studies of the sort conducted by Johnson (101, 102) that conditional or transition error probabilities (TEPs) are not equal from word to word, and that TEPs rise significantly at phrase-structure boundaries, i.e. that such error probabilities are larger between than within constituents, suggesting that sentences may be learned and remembered in terms of phrase length units. Or consider some work on sentence perception under conditions of noise (Mehler & Carey 148). These investigators attempted to establish a set for one or the other of two surface structures by presenting ten sentences of the one type or the other in the presence of white noise, and then presented a test sentence of the same or of different surface structure from the preceding sentences. The results indicated that a sentence was recognized significantly less accurately when it followed a series of sentences different from it in surface structure than when it followed a series of sentences with the same surface structure. (Results from a similar study concerned with the effects of changes in base structure on sentence recognition were very marginal at the most.) Many other examples regarding the important role of surface structure information may be found in Bever (13), in Garrett & Fodor (79), and in Wales & Marshall (197).

We turn next to some studies on the subjective (shift in the) location of a click heard while listening to a sentence. These studies were first interpreted as demonstrating the salience of surface structures in the perceptual analysis of sentences (e.g. 72, 79) but latterly have been interpreted as evidence for a perceptual strategy of isolating lexical sequences in the surface structure which correspond to sentences in the underlying or deep structure (see e.g. 13, 14, 16). This research has dealt with the perception of nonspeech interruptions in sentences with two clauses, and the principal finding has been that subjects report a single intruding click in a location displaced toward the break between the two clauses. There is evidence (78) that this systematic effect of syntactic segmentation is not due to actual pauses or to some acoustic cues in the sentence since one may obtain systematically different displacements of a click in a given location in the same sentence segment as a function of the differences in the syntactic structure of the sentence (manipulated by appropriately embedding the sentence segment in different surrounding materials). There is evidence (16) that within-clause surface phrase

structure boundaries are not effective in attracting click-errors, and that the most effective structural division governing errors in click location is the point corresponding to the division between sentences in the underlying structure (and that such results obtain even in the absence of corresponding explicit clause divisions in the surface phrase structure). The by now considerable body of "click" literature is interesting in revealing how the same experimental situation may be used to probe more and more deeply into a problem, and how the whole direction of interpretation may change as this happens. One cautionary observation must be made: in the click experiments subjects report on the location of the click *after* hearing and writing down the sentence, so that it is not at all clear that the effects can be considered to be perceptual effects rather than memorial effects, or as revealing of delayed response strategies. Seeking to cope with this sort of criticism, Bever and his associates have used GSR to shock (15) and immediate reaction time to a click (2) as more direct and immediate measures of a subject's response to speech as he hears it. While the results of these studies indicate that the syntactic structure of a sentence can systematically influence the GSR to shock and reaction time to clicks, and that therefore syntactic knowledge may be used actively to continuously modulate attention during listening, these studies also yield so many complex (and often unanticipated) findings, and involve such a high ratio of inferences and hypotheses to data (see particularly 2) as to leave matters very unsettled indeed. Perhaps some of the results may be more directly attributable to special strategies due to the unusual experimental situations, involving features disruptive of normal speech perception, than to the normal processes the experiments sought to tap.

Let us note one other example of work explicitly directed to revealing the importance of deep structure properties in sentence memory (18, 21). Blumenthal asked subjects to commit a set of sentences to memory, and then had them attempt to recall the sentences, each sentence being now prompted by a single word. In the first study the sentences all had the same surface structure, but differed in deep structure in that some sentences were standard passives, others consisted of passives with the agent by-phrase replaced with a nonagent by-phrase (e.g. "Gloves were made by tailors" vs "Gloves were made by hand") such that final nouns in the former case served as logical subjects, but in the latter only as adverbials. It was found that nouns in the former case involving the more inclusive subject-of-sentence relation were significantly more effective prompts than the modifier nouns in the latter case. In the second study, distinguished for the care taken to ensure full initial understanding of the sentences involved, it was found that for sentences matched in terms of surface structure, a noun prompt which was the logical subject of the sentence facilitated recall more than a noun prompt which was the logical object, and that sentence modifiers were better prompts than noun modifiers—again recall differences corresponded to the nature of the underlying grammatical relations among sentence parts. It

should be noted (cf. 198) that these results, which indicate that the more inclusive the grammatical relations the more effective the prompt, are direct evidence with regard to the psychological relevance of deep structure, as claimed by Blumenthal, only if the relation between deep structure and basic grammatical relations is one of identity, but that it is in fact "quite possible to suppose that assigning the deep phrase marker is not the means by which the basic grammatical relations are determined" (198), since deep structure displays many features irrelevant to the grammatical relations. For some very valuable further work on this problem see Wanner (198) where, among many other things, there is evidence that the effectiveness of a prompt is related to the number of deep structure propositions in which it is involved, rather than to differences in relational inclusiveness. [For some other relevant work see Levelt & Bonarius (118).]

Recently Fodor and his associates (71, 74, 75) have come to argue "that listeners actively use their knowledge of the underlying structure potentialities of particular lexical items in sentence comprehension," that the psychological complexity of a sentence is a function of "the degree to which the arrangement of elements in the surface structure provides clues to the relations of elements in deep structure . . . since understanding a sentence requires [among other things] recovering the grammatical relations that obtain among its parts," and that insofar as transformationally complex sentences are more difficult to process, it is because such sentences provide fewer surface clues as to deep underlying structure. In an experiment in which subjects were required to paraphrase doubly self-embedded sentences (74) it was found that versions which included relative pronouns ["The pen (which) the author (whom) the editor liked used was new"] were easier to deal with than versions where relative pronouns were deleted. It was argued that this was the case "because the relative pronoun provides a surface structure clue to semantically crucial deep structure relationships." By including additional experiments, as adding adjectives to the sentences, the investigators were able to rule out an interpretation of the results in terms of differences in derivational complexity, since such additions which increased greatly the derivational complexity of the sentences did not impair performance. In a subsequent study (75) it was shown that subjects' performance in paraphrasing sentences and in solving anagrams was significantly poorer for items involving more complex verbs. (A verb is considered to be more complex the more different deep structure configurations it may enter in or dominate.) It is argued that the deep structure properties of a verb play a major part in the listener's determination of the underlying structure of an input sequence, and that "the greater the variety of deep structure configurations the lexicon associates with the main verb of a sentence, the more complicated the sentence should be." Thus what is taken as crucial is not just the actual realized structure of a sentence but the "structural potentialities of the understood lexical items," with particular stress on the importance of verb type—"the exploration of the lexical analysis of the main verb of a

sentence is a central heuristic in the strategy subjects use to recover its deep structure" (75).

In commenting on this analysis, Watt (200) points out that it seems to involve two somewhat distinct proposals: (a) a theory of cumulative assignments (TCA) which "maintains that as we process a sentence we tentatively assign to it a surface structure consistent with as much initial string as it takes to *uniquely* determine such assignment" and that "the more quickly the correct surface (hence deep) structure can be assigned (the fewer constituents it takes to support a unique assignment) the more easily parsed a sentence is"; and (b) a theory of verb dominated direct inductions (TVDDI) which holds that in processing a sentence one makes "direct inductions" of a set of "base configurations," on the basis of how each verb is classified in the lexicon, where (b) assigns special processing importance to verbs and makes some specific assumptions as to how their processing may play the heuristic role assigned. While arguing that on the whole TCA provides a more plausible account for some cases than TVDDI, Watt attempts to show by means of a consideration of a large number of instances, but particularly via an analysis of truncated passives and the relation of these to full passives (on the plausible assumption that the former are psychologically less complex than the latter), that the sorts of suggestions made by Fodor et al just will not work in general. In other words, with regard to any differences in psychological complexity between full and truncated passives, TCA cannot help "since every full passive that fully corresponds to a truncate (of n words), *includes* that truncate as its first n words: any TCA-predicted delays in assigning surface and deep structure must affect such sentences equally." While Watt's objections to the particular proposal or proposals considered appear compelling, it is not clear what their bearing is with regard to the very rich and varied assemblage of processing strategies suggested and synoptically reviewed by Bever (13, 14), or to what extent his analysis is compatible with some of the principles suggested by Bever.

SEMANTICS

While recently there has been a renewed interest among linguists in problems of semantics (108, 202), and while recent developments in transformational grammar have come to be more and more concerned with the intimate relation between syntax and semantics, it is clear that semantic theory is in a very rudimentary state (as compared to syntactic theory), and that therefore the psychologist interested in problems of semantics is not heir to most of the benefits (and pitfalls) consequent upon the availability of a well-developed and articulated linguistic account. Indeed "given the underdeveloped state of semantic theory, it is hardly surprising that even the best papers on performance aspects of semantics give the impression of trying to solve one equation in two variables" (141). Here we shall restrict attention to a consideration of some recent attempts at the analysis of word meaning, usually carried out by studying similarities or relations in meaning, and a

presentation of some recent work on the use of the semantic strategies in understanding and memory for sentences. For recent valuable reviews which examine some of the basic conceptual issues involved and consider problems of methodology as well as substantive findings see (57, 141, 150, 169); for an attempt to provide a basic system of categories see (113, Appendix), and for a disappointing review of psychological work on meaning see (52).

THE ANALYSIS OF WORD MEANING

A basic assumption of recent work is that "the meaning of a sentence is derived from the meanings of its constituent phrases which in turn are derived from the meanings of the words which compose them" and that the meaning of each of the words involved is a compositional function of some more primitive semantic features or components. Work on the lexicon is obviously of significance for its own sake, and also has direct relevance with regard to work in syntax since "evaluations of the adequacy of proposals for deep-structure syntax are impossible in the absence of well-confirmed lexical representations," and since the very assessment of the significance of psychological studies of syntax depends upon an adequate control of lexical relations in the experimental materials used (141, 197).

There have been a number of attempts by psychologists to specify or isolate conceptual features involved in the meaning of individual words or sets of related words, i.e. studies directed to determining how the psychological lexicon may be organized and stored in memory. Miller (150, 151) had subjects sort words into clusters on the basis of "similarity of meaning," assuming that all the items involved were conceptually distinct for the judges, that in grouping them they must necessarily ignore some of the distinguishing features, and that by an analysis of the sortings one might be able to discover "which conceptual features have been ignored and thus by indirection, what the features are" (151). As his materials in one study (151), Miller chose 48 nouns "selected rather arbitrarily to cover a broad range of concepts subject to the constraint that half of them should be names of objects, and the other half names of non-objects." Upon subjecting the sorting data to a hierarchical cluster analysis, Miller found something like five major clusters, and he argues that a close scrutiny of the results suggests that item sorting was carried out largely on the basis of the common definitional presuppositions of the nouns (where the presupposition of a noun is an identifying superordinate class name appropriately specified or restricted in the rest of the definition, e.g. "a knight is a *man* who has been raised to honorary military rank"). Miller (150) has applied a clustering analysis to a re-examination of some word association results obtained by Deese (55), to a further study of some of the words used by Deese, and also to a further examination of some sets of antonymous adjectives, following up some earlier work of Osgood. The results revealed the usefulness of the classification approach to the study of closely related lexical items, and in both cases considerably clarified previous findings. However, the results of similar

work on a set of verbs were very difficult to interpret and appeared to be very sensitive to context, leading Miller to the suggestion that the semantic analysis of predicates might be basically different from that of subjects, and that verbs might signify some rather special formulae, "complex functions into which particular nouns can be substituted as arguments." This notwithstanding, the classification method appears to be a promising one for discovering some of the important features involved in the subjective lexicon, and for saying something as to the nature of their organization. If one asks about the nature of the *structural organization* of semantic features in a particular case, whether it is linear, hierarchic, paradigmatic, or whatever, one raises some major and essentially unsolved conceptual problems, and also measurement problems with regard to the sorts of techniques that might be used to analyze findings and to evaluate the adequacy of various structural models—for some comments on these matters, see especially (154; also 57, 151, 179).

Osgood (154) has made an extensive attempt to provide an account of the semantic structure of a large set of interpersonal verbs and adverbs, employing a judicious combination of intuitive and empirical methods. He sought to provide an analysis of these words in terms of a set, finally, of ten a priori semantic features intended to capture the most important meaningful properties of the verbs, and to maximally discriminate among them. He found that subjects could use the features to discriminate semantically among the verbs, and did so in almost total agreement with prior feature ratings made for each verb, and that when a variety of empirical tests were made of the way in which these verbs were understood and used, something corresponding roughly to five or six of the features characteristically emerged. One of the methods he has developed appears of particular interest: this is a *semantic interaction technique* where inferences as to underlying semantic features are made in terms of whether syntactic combinations of words are judged to be apposite, anomalous, or simply permissible, on the assumption "that the bundles of semantic features of word elements can be inferred from their rules of usage in combination" (155). Thus, regularities in the acceptability vs absurdity judgments of speakers about sets of interpersonal verbs in sets of adverbial frames are used to infer the semantic features of both sets. The monograph in which many of the results are reported (154) is very rich substantively, and also raises many basic conceptual and methodological questions—see e.g. the discussion of "the trouble with trees" regarding some difficulties with Miller's approach mentioned above. Nevertheless, for the particular case examined, we are still left with some considerable problems, particularly with regard to the adequacy or sufficiency of the features in marking important differences in meaning. For example, in terms of the set of ten features, ACCUSE, BLAME, and CONDEMN are equivalent, and as a group differ from CRITICIZE only in that the latter is marked "+" on the Active/Passive and Deliberate/Impulsive features, while the other three terms are marked "0" on these features

(where + or - indicates that a particular verb has the given feature in its positive or negative form respectively, and 0 indicates that the verb is not differentiated by that feature); While one might agree that the first three verbs mentioned are very similar in many respects, yet one would surely boggle at treating them as equivalent, and one may certainly wonder whether CRITICIZE really differs from the aforementioned verbs principally in the respects indicated (for an alternative linguistically motivated analysis of such verbs of JUDGING see 66). Also it is not clear how these features are supposed to be organized, and the data obtained by the various empirical procedures do not permit any very coherent account of this, perhaps in part because of the enormous "territory" covered by the 198 terms involved (or even by a heterogeneous subset of 40 of them studied in greater detail).

Recently Steinberg (193) has employed concurrently a word sorting task and a task where, on the basis of a few examples only, subjects were required to categorize simple sentences of the form "the x is a y " as informative, analytic, contradictory or amphigorous (nonsensical). He found that subjects were able to sort the sentences into these (implicitly defined) categories—hardly a very surprising result for the sentences considered—and that by using conjointly the results from the two procedures, he was able to establish a hierarchical organization for the semantic properties governing the subjects' judgments, and to account for the findings in terms of some feature interaction and sentence interpretation rules. The lexical items employed in the study were chosen on an "intuitive basis in order to yield a variety of similarities and contrasts with respect to postulated underlying semantic dimensions," and it seems clear that the method employed was able to recover and display the structural relations deliberately built into the set. Whether it could serve as a discovery technique, given a more heterogeneous set of items where the semantic properties involved and their organization are unknown, is very much an open question.

Let us now note work by students of Deese directed toward an experimental investigation of some coherent semantic fields or domains. Henley (91) conducted a study of the presumably highly structured domain of English animal terms, using a variety of techniques to obtain her data (free listing, pair and triad ratings, free association, paired-associates learning) and to analyze her data (factor analysis, hierarchical clustering analysis, multidimensional scaling). With the exception of the data from the paired-associates task, the different techniques yielded consistent results, revealing a fairly clear structure for the terms and yielding dimensions such as size and ferocity, and very sensible clusterings of the terms. In a study concerned with the semantic structure of a subset of English kinship terms, data were obtained through use of a short-term memory task (92). Recall was better when fewer semantic features distinguished the three terms of an item, and there were more intrusion errors for items differing in all three features than for items differing only in two features. For some other psychologically oriented work on the structure of English kinship terms see Sanday (173),

who used an information processing approach to analyze her listing data, and Wexler & Romney (201), who used a triad similarity task to generate the data by which to test two explicitly formulated cognitive models for English kinship terms, coming up with results decisively supporting a model involving a reciprocity dimension over an absolute generation model.

PROCESSING STRATEGIES

To this point something has been said regarding the analysis of word meaning into some more elementary components. Now we shall note some work on memory for semantic, lexical (and syntactic) aspects of connected discourse. Sachs (170, 171) told subjects that they would hear a short story after which they would be asked to judge whether a given sentence was identical to one they had heard or whether changed, and if changed whether changed in "meaning" or "form." The test sentences were unchanged or differed either in meaning or form from the original, or in terms of the substitution of a synonym for a lexical item. If the test sentence was presented immediately after the original sentence, retention was high for all test types; after 20 syllables of interpolated material, while recognition of semantic changes was high, recognition for other sorts of changes was already considerably reduced; and by 40 syllables (and thereafter), while recognition of semantic changes remained high, recognition of changes in voice, stylistic changes, and lexical changes was quite poor. Sachs argues that the original form of a sentence is held only for the short time necessary for comprehension, and that once a semantic interpretation has been made the meaning of the sentence is stored. In fact, subjects were able to retain some stylistic information about the sentences, but as Sachs notes, her subjects knew that they would be interrogated about the materials so that these results can only provide "an upper limit to the ability to recognize form after the intervals tested." What if one does not so warn the subjects, realizing circumstances more like those of normal comprehension? In a very ingenious experiment, Wanner (198) tested for incidental memory of a sentence in the instructions, and found that after some 50 interpolated syllables, recognition of a semantic change was high (nearly 100%) whether subjects had been prewarned or not, whereas recognition for stylistic changes was substantially lower even when subjects had been prewarned, and no different from chance for subjects in the incidental or unwarned condition. These results strongly suggest that while meaning relevant information is retained well under normal conditions of comprehension, little if anything is remembered about the superficial structure and stylistic properties of the materials. However, there are some quite recent results reported by Begg & Paivio (7) which indicate that under conditions where subjects are warned in advance that their memory for sentences will be tested, it makes a difference whether a sentence is made up of concrete or abstract materials, with semantic changes detected significantly more often than lexical changes (essentially quasi-synonym substitutions) for the former case, and the reverse true for abstract sentences.

We have just cited findings indicating that meaning content may survive rather well; such findings suggest something about the form in which sentences may be coded and stored in memory. Now we shall comment on the possible role of semantic strategies in sentence processing, and then look at work which focuses on some semantic properties relevant to memory and thinking. There is evidence that one important semantic strategy is just to combine lexical items in the most plausible way, that unique semantic constraints may allow the listener largely to by-pass syntactic factors, that "the most likely semantic organization among a group of phrases can guide the interpretation of sentences independently of and in parallel with perceptual processing of the syntactic structure" (Bever 13). Thus, using both children and adults as subjects in a sentence verification task, Slobin (182) found that differences in difficulty between active and passive sentences were essentially eliminated when he presented for verification pictures of nonreversible situations (i.e. situations where the object of the action could not normally serve as subject). Here syntactic processing could be short-circuited (for some further evidence on this, showing a marked increase in dependence on semantic constraints during the third year, see Bever 14). There are also findings on the comprehension of self-embedded sentences which reveal that these are easier to comprehend when the relations between subject-verb-object are semantically constrained. For example, "The bees that the hives that the farmer built housed stung the children" is easier than "The wagon that the auto that the truck carried pulled caught fire" (176, 194). It seems very plausible that the effects of such semantic constraints will be even more important under normal conditions of sentence processing, when there are available to the listener both external situational supports and longer range constraints from the discourse in which a particular sentence is embedded.

Some recent work by Clark seeks to demonstrate that memory for sentences has a semantic basis, and it attempts to characterize the form such knowledge takes and the processes by which it is retrieved. Essentially the view is that at recall the subject tries to reconstruct a sentence from the information still available to him, and that the way in which it is stored and retrieved will determine how quickly he solves problems requiring that information. Clark seeks to specify some of the properties of the semantic features involved, arguing that these may be either simple in form (unmarked) or complex in form (marked), that the features and their marking can be forgotten independently, and that memory loss causes errors to go toward the semantically simpler or unmarked forms, resulting in systematic simplification (41-43, 45-47). A central notion in all this work is the distinction between marked and unmarked terms (where the unmarked term is simultaneously the label for a category and the label for one member of that category and the marked term can serve only the latter role as e.g. in the contrast between *man* and *woman*). This involves the assumption that in the abstract representation of the information the marked form is always more complex by one additional feature, and that memorial processes are such that

this additional feature may be lost independently, with the result that at recall there is a systematic drift toward the simpler unmarked form. This principle of lexical marking plays a very significant role in interpreting the results of all of Clark's studies, and it is important to note that there is independent linguistic evidence for such a principle (see e.g. Greenberg 86).

This approach was applied in the study of memory for (a) complex two-clause sentences involving the ordering of events (46); (b) sentences containing verbs of varying form—either present or past, progressive or non-progressive, perfect or nonperfect (47); and (c) comparative and equative sentences phrased either positively or negatively and containing either unmarked adjectives (like *good*) or marked ones (like *bad*) (45). With regard to (a), sentence confusions indicated that subjects remembered three semantic distinctions: temporal order, order of mention, and the main-subordinate relation of the two described events. The results could be accounted for if one assumed that order of mention may be marked (if the sentence is such as to indicate that the event mentioned first actually occurred second), and that the main-subordinate relation may be marked (if the subordinated event is mentioned first), and that on recall there is a shift toward the unmarked forms. With regard to (b), on recall verbs underwent semantic simplification, e.g. perfect verbs were recalled as nonperfect, progressives as nonprogressives, changes being such as to entail loss of a semantic feature, where the semantic features of a verb may be regarded as restrictions on its interpretation which more specifically delimit the action described. In (c), subjects also made systematic errors mostly predictable from a semantic feature theory, e.g. often reconstructing unmarked from marked adjectives and positive sentences from negative ones. It should be noted that the results of none of these studies could have been accounted for adequately by any theory which is simply syntactically based.⁴

In some further work on the comprehension of comparatives and the uses of linguistic processes in deductive reasoning, particularly in the solution of three-term series problems, Clark (41-43) examined subjects' performance in answering questions about the information available in sets of comparative sentences. He sought to interpret the results in terms of a theory based on three principles: (a) a principle of the primacy of functional relations which asserts that functional relations defined in deep structure like those of subject, verb, and direct object, which are the primitive conceptual relations out of which sentences are constructed, are stored immediately after comprehension in a form more readily available than other less basic kinds of information; (b) a principle of lexical marking according to which unmarked forms are stored in memory in less complex and more

⁴ For some questions as to whether difficulty in recalling the exact form of a sentence reflects difficulty in comprehension, see Smith & McMahon (190), which presents results for a task such as that employed by Clark & Clark but using question-answering techniques.

accessible ways than their correlative marked forms—this principle being the one central to the interpretation of the memory studies considered above; and (c) a principle of congruence according to which the listener can retrieve from memory only information congruent, at the level of functional relations, with information asked for in a question, implying that retrieving an answer should result in fewer errors and take less time when the propositions are congruent in their base strings than when incongruent. Thus the first two principles “specify what it is that the listener knows of a sentence he has heard [while] the third specifies how he searches his memory for the wanted knowledge.” These principles are used by Clark as a basis for predicting the relative times, frequencies of errors, and nature of errors in solving two-term and three-term series problems (e.g. “If John is better than Pete, and John is worse than Dick, then who is the worst?”). Not only is the theory successful in accounting in some detail for a rich variety of results, but it is shown that a number of alternative proposals (as a theory of spatial paralogic or a theory of spatial images) fail at one place or another, and that the theory is applicable more generally to other reasoning problems, in particular those requiring subjects to judge the truth or falsity of positive and negative statements. Clark’s approach stresses the importance of various abstract entities (closely related to certain linguistic facts about deep structure and the organization of the lexicon) in memory for sentences and in deductive thinking. He argues that “the principal difficulties inherent in many reasoning problems are not due to cognitive processes specific to those problems, but to the very language in which the problems are stated . . . that reasoning is accomplished mainly through certain very general linguistic processes, the same mental operations that are used regularly in understanding language” (42). Thus his position comes close to being one which regards reasoning, or a certain kind of deductive reasoning at least, as being very largely equivalent to the understanding of the language of certain statements. Clearly this argues for a very intimate relation indeed between language and (certain forms of) thought—for some general comments on this issue see Jenkins (98).

Some further evidence on the relation between the linguistic form of a sentence on the one hand, and how it is understood and reasoned with on the other, may be found in some work by Johnson-Laird on the understanding of logically complex doubly quantified sentences, and on reasoning with them (105, 106). Such sentences as “All philosophers have read some books” and “Some books have been read by all philosophers,” are equally ambiguous depending on the interpretation of the quantifiers, yet the first seems more likely to receive the interpretation “All philosophers have read some books *or other*” and the second the interpretation “All philosophers have read some books *in particular*”; thus the likelihood of each interpretation may be systematically affected by the particular phrasing of the sentence. When subjects were required to classify diagrams representing different specific situations as truthfully or falsely described by such sentences, it was found that

the order of occurrence of quantifiers made a critical difference, causing actives and correlative passives to receive different interpretations—with “some” being emphasized and understood as “some in particular” in sentences where it came first and served as grammatical subject, more than in sentences where it appeared second and served as grammatical object and was more likely to be interpreted as “some or other” (106). The results of this study provided a good estimate of performance on an inferential task where subjects had to judge as valid or invalid inferences from such doubly quantified sentences (105). Inferences from a premise with “some” in the grammatical subject to “some” in the grammatical object were likely to be judged as valid, while inferences in the opposite direction tended to be judged as invalid, with the position of “some” being critical in the evaluation of the inferences. There is much more work of this genre by Wason and by Johnson-Laird concerned with whether the formal structure of rules or the words in which they are expressed is responsible for difficulties in reasoning, particularly with regard to reasoning about disjunctive rules (199). Here we cite only one further study, “How implication is understood” (107). Subjects classified stimuli as to whether they indicated that a sentence was true or false (or irrelevant to its truth value). Four different sentences were used, with each logically equivalent to a material implication. Disjunction (not- p or q) was most often classified as material, implication, the remaining sentences (if p then q , not- p if non- q , never p without q) were not classified in a truth-functional way, stimuli being judged irrelevant if they falsified the antecedents of these sentences, with substantial differences in interpretation among these latter sentences also. Thus the principal finding is that the way “in which implication is expressed exerts a decisive influence upon what it is understood to denote.”

Work by Abelson and his associates (1, 80) on “subjective verbal logic” used inductive and deductive evidence forms, with subjects given positive and negative instances and required to say whether or not a conclusion or general assertion follows on the basis of the evidence presented. The findings of the first study (80) revealed very clearly “that the tendency to accept a generic assertion based on inductive evidence depends almost entirely upon the verb of the assertion and hardly at all upon the subject and the object,” with verbs differing sharply in their generalizing power. The same finding holds for deductive evidence forms, except “that verbs having a high generalizing power when used with low-level concrete (inductive) evidence tend to have low generalizing power when used with high-level abstract (deductive) evidence, and vice versa” (1). Departing from some ideas of Heider, Abelson differentiates between verbs representing negative subjective states (e.g. *fear*) which have very high deductive power and low inductive power, and verbs representing manifest negative actions (e.g. *fight*) which have fairly high power both inductively and deductively, and between verbs representing positive subjective states (e.g. *like*) which have fairly high inductive power and low deductive power, and verbs representing positive episodic

interactions, i.e. positive manifest verbs (e.g. *buy*) which have very high inductive power and low deductive power. It seems clear from these results that "verbs trigger subroutines." What is not clear is why different verbs have the different effects they do. At best the cross-classification into subjective or manifest, positive or negative verbs may have some value as a summarizing and heuristic device, but, as the authors admit even while offering some speculations, we really do not know what general principles might account for the differences in results with different verbs.⁵ Obviously what is required is a deep semantic analysis of verbs, their presuppositions and logic, something which is now available only in the most fragmentary way for a few instances [see e.g. Fillmore's analysis of some verbs of JUDGING (66)].

USES OF LANGUAGE

We shall now, finally, touch on the uses of language in describing and in interpersonal communication. In a recent fascinating and important paper on "simply describing" or "where sentences come from," Osgood (156) considers the antecedents of sentences and shows how the form and content of a sentence may be affected by manipulating the perceptual context in which it is produced, how nonlinguistic perceptual events can create cognitive presuppositions which can influence the forms that sentences may take. His position is that prelinguistic perceptual-motor behavior shares many (if not all) of the characteristics of linguistic behavior, that the ability to paraphrase perceptual-motor events in language (simply describing) implies that perceptual events and signs must have meaning and structure, that perceptual and linguistic signs must, at some level, share a common representational system and a common set of organizational rules, that what is shared by both sign systems is not linguistic but cognitive in nature, and that this nonlinguistic system is "where sentences come from." To assess the effects of perceptual events upon the forms of sentences describing them, Osgood compares sentences generated by events in which a given cognitive presupposition may be assumed to be operating with sentences generated where it is not operative. Subjects described simply some fairly elementary events staged by Osgood in cunningly chosen sequence. Categorizing verbs with regard to time as *action* or *state*, *terminal* or *interterminal*, he shows how events described by interterminal verbs (e.g. *hold*) are more likely to be phrased

⁵ In a paper on "Language and Attribution Theory" given at a very recent conference, Kanouse has suggested that differences between subjective and manifest verbs may be due to differences in implicit quantification, with subjective verbs implicitly quantified "all," and manifest verbs implicitly quantified "some"—thus "John hates fruit" is understood as "John hates *all* fruit," while "John buys fruit" is understood as "John buys *some* fruit." While an analysis of this sort does indeed lead to a considerable clarification of experimental results, it is not entirely clear why there should be such differences in implicit quantification; but see Kanouse for some suggestions on this.

in the present than in the past tense and to involve use of a form of *to be* than are verbs describing terminal events (e.g. *put*). He demonstrates that "the perception of entities as novel or as unique or as requiring differentiation from others" has "predictable effects upon a range of linguistic phenomena . . . determiner, pronoun, reflexive, adjective and adverb usage," e.g. the indefinite "a" is more likely to be used in identifying something new and characterizing a member of a set, while the definite "the" is used for re-identifying or recognizing something already encountered, or to refer to a unique event. The data reveal that modifier use declines when a known event recurs and increases when an event must be distinguished from others, etc. The paper is a very rich one in which various other effects, as the ways in which uses of the negative are conditioned by the occurrence of an event not expected or predicted, and the dependence of tense upon the relation of event time to a subjective time origin, are presented and discussed. The paper is unique in that there is hardly any other linguistically sophisticated (or even unsophisticated) experimental work available on a matter as obvious and as important as the uses of language in describing, and for the lesson it reads with regard to the critical importance of nonlinguistic cognitive presuppositions on the form and content of such descriptions.

There is work too on the ways in which some aspects of the communicative situation may affect properties of a description (111, 112). Pairs of subjects were given a problem-solving task that required them to communicate about novel, ambiguous figures. It was found that the reference phrases used in these descriptions involved assimilations to common objects plus appropriate qualifications (e.g. "looks like an hourglass with legs on each side"), and that the length of the reference phrase decreased as a function of repeated reference (cf. Osgood, above, on the elimination of modifiers upon recurrence of a known event). Further (112), it was shown that there was less abbreviation in the absence of concurrent feedback, and with confirmation of the success of the message only part of the time (rather than on all trials). Clearly efficiency of the initial communication via denotative convergence by "an exploitation of the natural language of familiar objects" (166), and subsequent abbreviation, once communication has been successfully established, both reveal how the language of description in such a situation is a product of social interaction. Young 4- to 5-year-olds confronted with a similar task involving novel items do very poorly (83), largely because they seem to use privately relevant descriptions which fail to communicate, as though they were lacking a common cognitive nonlinguistic world in terms of which to cast their descriptions, and in the ability to take the role of the other.

PROCESSING IN REAL TIME

We have largely ignored temporal phenomena, except insofar as we have noted that latency measures have been used to index difficulty in processing. Yet normally "we must operate in real time," and temporal phenomena

play an "all-important role" in language, as has been noted by Lenneberg (115) in considering problems of rate and ordering, and problems of timing at all levels, and in arguing that patterning in time is based on an underlying rhythmic metric (see also 90 on "cognitive rhythm"). In the early 1950s, Goldman-Eisler initiated a program of research on spontaneous speech seeking "to gain an understanding of the generative processes involved in speech production" through a study of some temporal parameters of hesitation pauses and breathing—much of this work is summarized in her 1968 monograph (84). It appears clear that properties of pauses, and of their location and distribution, are informative of cognitive processing, "that decisions of a lexical and structural kind as well as of content made in the course of speech utterances" are accompanied by pausing; for example, pausing time when speakers interpreted the meaning of cartoons was about twice as much as when they were merely describing their content, and pausing varies with different degrees of spontaneity. Hesitations in speech clearly indicate something important about psychological processes involved in encoding. One critical distinction to be noted is that between conventional and idiosyncratic pauses. Conventional pauses which occur at breaks in the intonation pattern and between major sentence constituents, i.e. at syntactically appropriate locations, may be regarded as revealing of the speaker's knowledge of the grammar (as indicative of linguistic competence), and as the places at which syntactic planning occurs primarily. Idiosyncratic pauses, which are particularly common in spontaneous speech, and characteristically fall within major grammatical constituents, may be considered as revealing of the speaker's cognitive activity in semantic choice and encoding (and as therefore particularly relevant to problems of speech performance). For example, Goldman-Eisler and her associates have shown that in reading aloud, where little if any semantic planning is required, people pause rarely, and then characteristically at syntactic junctures. On the contrary, in speaking spontaneously, hesitations and breathing occur more and at places other than syntactic junctures, and the pattern of speaking and pausing is strikingly different from that in reading (89, 90). There is much more evidence of this kind suggesting that hesitations may be revealing of the operation of syntactic and semantic processes, and that they are sensitive to properties of the speech task. One observation needs to be made about this work, and much other work on temporal phenomena in speech, namely that such work either has been devoid of reference to linguistic analysis, or has made use of only weak analyses. Thus Goldman-Eisler measures complexity of sentence structure by the proportion of subordinate clauses to the total number of clauses (without any further analysis), and indexes grammatical juncture in terms of a somewhat unmotivated (and largely unexplained) set of criteria. Martin (145), to take an extreme but not atypical example, bases his linguistic analyses on some differences between content and function words. While spontaneous speech has many properties that make it recalcitrant to

linguistic analysis, the application of such analysis is nevertheless essential if one is to arrive at a fuller understanding of pausal phenomena, and thus of the processes involved in normal speech.

ENVOI

I must close with a *mea culpa*—the necessity for selectivity has resulted in the omission of major topics such as sociolinguistics (see e.g. 63), quantitative psycholinguistics (see e.g. 28), studies of language defect (see e.g. 122), studies of reading (see e.g. 119), studies of bilingualism (see e.g. 129), and cross-cultural work (see e.g. 94), and many significant papers have not been considered.

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MASS COMMUNICATION¹

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The impression gained from a review of the literature—for the 3-year period beginning in January 1967—is that at this time the area of mass communications lacks conceptual coherence and comprises principally a potpourri of diverse research interests. It is a borrowing area, borrowing theory and principles small and large from the social sciences generally. But then, this should not be surprising, since the research focuses on the significant role played by the instruments, contents, and effects of public communications in the lives of people and the functioning of sociopolitical systems. Similar thoughts were expressed by Tannenbaum & Greenberg (162) in the opening of their review in the 1968 volume of the *Annual Review of Psychology*. Since then, customary lines of inquiry have been pursued by a multitude of investigators who have made useful but discrete additions to knowledge. Despite the existence of centers for research on mass communications, mainly university connected, systematic, coordinated, and continuing endeavors by groups of investigators, so needed for substantial advances on broad fronts, are only occasionally to be found.

This review is of course selective in regard to both the topics covered and the literature cited. The principal topics omitted, although touched on here and there, comprise communication studies related to advertising and consumer behavior, historical, structural and organizational analyses of the media, experimental research on attitude change, methodology, and the educational-instructional uses of the media. A handbook would be needed to encompass all that can be assigned to the portmanteau term of mass communication. The major dimensions and topics of the review are similar to those in a previous one by Weiss (175), and the cited literature picks up chronologically from the termination point in that coverage.

Brief mention should be made of the significance of communication satellites, for their further development and wider use promises to open a new era of potentially revolutionary changes in communications (143, 144, 169). But realization of their full potential, when direct transmission to home receivers becomes possible in 10–20 years, is much less likely to be impeded by purely technological matters than by economic, legal, and especially socio political obstacles (23, 143, 144, 169). A preview of thorny

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questions of national interest and sovereignty is already evident in Canada's concern over the impact on her national culture of American media products such as news services, television programs, and movies (8). Also suggestive is Hollander's (82) examination of the changes in Soviet radio and television news reporting after the jamming of foreign broadcasts ended in 1963. Hence, the early initiation of worldwide planning and discussions on non-technological difficulties looms as a crucial need. But, as Schramm (143, 144) points out, the proper perspective concerning the allocation of limited resources to new media or media developments should be in terms of the supplementing contributions they can make to previously determined societal needs (cf. 34, 145, 168).

INFORMATION FUNCTION

Preferred sources.—Data on the public's assertions about the relative utility of different media as sources of information are of general significance for an understanding of the uses and functions of the mass media. Essentially this comes down to the relative standing of television and newspapers, since magazines and radio usually place far behind these two as first choices. Television is chosen by the general public over the press as a source of general information (134), or for stories of greatest general interest (18), or when the visual element is an inherent quality of a story (171), or when the story has more than local significance (134). But the press is preferred to television on most of a large pool of specific stories, including science and health news (18, 171), or when the visual element is not central or the event must be interpreted (171), or when the story is of local interest (18, 29, 69, 134). But, except for very specialized topics, most persons (particularly the better educated) think of and use more than one source (18, 171). Other people are rarely cited as preferred first sources of information, although Greenberg & Dervin (69) report some evidence of an increase in mentions of such sources among low-income people. It should also be kept in mind that media actions can serve the function of communication from one part of the public to relevant others (29).

Media credibility.—The trend of research evidence in recent years is quite consistent in establishing television as the informational medium of highest credibility, given an intermedia conflict in news versions; newspapers are second and other media lag behind (16, 30, 68, 70, 84, 104, 134, 158). Roper (134) reports that television's advantage over newspapers has been increasing steadily so that by 1968 it was chosen by twice as many people as chose newspapers. Chang & Lemert (30) report that, even for a higher than average educational sample and even for those who use only the press for news, television is considered more credible than newspapers. The general pattern of television's greater credibility appears to be transnational in significance and does not appear to be due merely to differential usage of the media (16, 158).

Reasons offered by respondents for their choices of most credible medium emphasize television's visual and live qualities and the press's thoroughness in reporting (30). But, in addition, a perception of the differential bias of the media comes into play (30). This seems to hit newspapers hardest, for they are viewed as less objective regardless of the medium ranked first in believability (84).

News selection.—Obviously relevant to an analysis of the information function is an examination of the factors that influence the selection of news stories for publication or broadcast. Such knowledge could preclude unwarranted criticism or reflexive defense of the news media and set trust or mistrust on a more rational basis. Since television has borne the brunt of criticism recently, an awareness by the general public of the process and problems of televised news broadcasting (105, 157) would have sharpened some criticism and modified others.

That television does not merely show reality, even in live broadcasts, has been clearly established (92, 93, 105, 177). Rather what is created as a result of an active selecting and interweaving of images is a kind of processed reality. An informative analysis of television coverage of the Kennedy assassination weekend by Love (102) goes beyond the fact of the creative process to the factors underlying the selection of transmitted events. While news judgment was the pervasive control, it was informed by such influences as the social context of news organizations and personnel, degree of access to relevant parties in the drama, a concern not to alarm the public, and, most significantly of all, the normative dimension of situational appropriateness. In a relevant study of factors underlying television news editor's judgments, Buckalew (24), using a Q-sort procedure, turns up the following five: conflict, significance, timeliness, proximity, and visual availability.

In regard to the press, the role of the publisher has been the focus of some recent research. Bowers (20) finds that publisher suggestions or instructions concerning news stories were more likely to be given when the paper's economic vulnerability to outside pressure was greater and when a particular story might be more likely to produce such pressures. Donohew (44) reports that in Kentucky, publisher attitudes on Medicare accounted for approximately 50 percent of the variance relating to differences in the extent and prominence of coverage given to Medicare and in the evaluative bias in Medicare news stories. The publishers' perceptions of their community's views did not significantly affect the results. Nor was there any support for the assumption that community characteristics likely to be associated with interest in and attitude on the topic (e.g. percent of population over 65) would be related to the quantity and bias of coverage. Shaw (150) also found no evidence that relevant community characteristics were related to local newspaper coverage of population and family planning stories. Rather, the emphasis given to such news by the major wire services was the major determinant.

Besides criticisms of inadequate or biased coverage, news media are often charged with inaccuracies in their news stories. The generality and validity of such charges are not only relevant to the training and function of newsmen but also to the general trust that the public places in news reports. In an examination of three San Francisco papers, Berry (15) found that only 46 per cent of the news stories were completely accurate. The most frequent inaccuracies were omissions, misquotations, or other errors of meaning. Although telephone interviews were often used by reporters, they yielded less accurate stories than did personal interviews. A similar finding is reported by Lawrence & Grey (97).

USES AND GRATIFICATIONS

Correlates of media use.—Educational level has been the most consistently significant correlate of media use. In regard to the print media, the relationship is generally positive (17, 18, 46, 71, 110, 128). This probably reflects the educated person's greater interest in the media as a source of serious information and 'hard' news (122), which shows up in his greater knowledge of public and world affairs (128). Since reading a newspaper is so widespread, unless the educational range sampled is extensive or unless care is taken to distinguish between different types of content, no relation may be found with a gross index of exposure (110, 140).

In regard to television, the relationship is usually negative (110, 120, 129, 140), and in contrast to a previous finding (138) it need not be due to the differential availability of leisure time (129). However, Greenberg & Kumata (71) report an inverted U-shaped function with the peak at 11–12 years of schooling; and Donohew & Thorp (46), using Kentucky's counties as sampling units, find a positive relationship between median county level of education and television usage. Of interest is Bogart's (17) report of a negative relationship between education and television usage for whites but a positive one for Negroes. Perhaps, as he conjectures, the Negroes in each of the two gross educational categories used in the study were much lower in both income and education than were the corresponding categories of whites. Robinson (129) finds that education is a positive correlate of television usage in Eastern Europe, where sets are not widely available and the programming is more like US educational than commercial television. Variation over studies in the likely significance of the contents of exposure, in the range of education sampled (and the use of suitable controls on other correlates), in the distribution of sets throughout the sample, in the nature of the sample, and in the indices of set usage probably account for divergences in results.

From the perspective of a functional viewpoint, research ought to focus on the psychological and motivational correlates of media usage. When such studies of uses and gratifications are carried out, the media or media content are usually viewed dichotomously as predominantly fantasy-escapist or informational-educational in significance; and the selected personality characteristics are those expected to relate more closely to one or the other

kind of material. In line with this approach, Rees (127) finds that degree of achievement need is positively associated with preference for magazines categorized as delayed gratification types, but is not related to preference for television programs of the same type. Other investigators (110, 140) report that a sense of alienation from other people and from society was generally unrelated to media usage. Poverty rather than anomie was the correlate in one study (140); and in the other (110), while the alienated were less interested than the nonalienated in nonsensational headlines, they were not more attracted to sensational ones. In sum, the results of these and other studies suggest that, at best, personality factors will account for relatively little of the variance of media usage and will generally operate to produce minor perturbations in overall patterns of preferences based on other determinants. This tentative evaluation is in contrast to the suggestion of Greenberg & Kumata (71) that nondemographic attributes should be employed, since even a cluster of the usual demographic ones controls only approximately 25 percent of the variance.

Leisure time.—Since exposure to the mass media is discretionary, it tends to occur during times free of required or obligatory behavior, although it does seem to appear with some frequency as a secondary activity (58, 118, 129, 175). Evidently then the amount of leisure time and the kinds of leisure activities pursued and available are significant determinants of customary media usage (112, 175). Hence, to provide the temporal-behavioral context into which the media fit, a mapping of the patterns of time allocations for obligatory and nonobligatory activities becomes necessary. The relevance of such information was noted in a limited study by Belson (11). But now more elaborate and systematic data-gathering has begun in a number of countries (129, 141). For instance, Robinson (129) reports that in the US leisure time is split about evenly between exposure to mass media and all other kinds of activities, with television occupying more than twice as much time as all other media combined. However, to forecast the media's future effects on leisure time also requires detailed, quantitative knowledge of the motivations and gratifications surrounding their use (58, 166).

For Dumazadier (49), leisure time is a central element in life and a need, providing opportunities for relaxation, entertainment, and personal development. Since the media are principally useful for and used for leisure, evaluations of media performance should be based on their contributions to the full variety of leisure uses and not on elite standards of 'high culture.' Stephenson's (160) emphasis on the play or pleasure element in the media experience is congruent with Dumazadier's (49) and Sato's (141) views. Of interest is Meyersohn's (112) report of a complementary relationship between extent of exposure to television and engagement in other leisure activities among the poor, but a competitive one among the well-to-do.

Television.—Recent data on the place of television in the lives of adults and children are provided mainly by Japanese researchers. Fujiwara (58)

finds that among adults the percentage of total viewing time devoted to educational and cultural programs varies only slightly by age or occupation; in contrast, the percentages for other kinds of programs vary more widely. The primary reasons for viewing television are relaxation-entertainment, a general cultural-informational benefit, and the pleasure of family viewing. [In a Canadian study, Wand (174) finds that, despite the incentive of togetherness, family viewing of television is not likely to occur when program interests are too diverse.] Compared to other media, television is more likely to be chosen to satisfy multiple desires.

In a summary of research on Japanese school children, Furu (59) reports further data documenting a transcultural turning point around the beginning of the teens, for a shift from fantasy-oriented to reality-oriented media content (175). For instance, in a longitudinal study, seventh graders showed an increase in attention to serious articles in newspapers compared to their interest when they were fifth graders. In conformity with data from other countries (81, 146), Japanese researchers find no evidence of television-induced passivity among children. Furu notes the problem for such research of the varied and unclear meanings of "passivity"; perhaps Dumazadier's (49) distinction between active and passive attitudes toward the media experiences themselves could prove useful as a refinement of exposure. Research is in progress on Himmelweit's (80) proposal concerning the use of school teachers to shape the television-viewing behavior of young people. Preliminary data suggest that recommended programs were watched, but that over the relatively short time interval used general tastes were not affected.

Furu also reports no evidence for a television effect on childrens' realistic or fantasy expectations about jobs. While not assessing the same type of effect, DeFleur & DeFleur (41) assert that television does contribute to children's knowledge of and regard for occupations experienced only through television.

Although not an example of research on the significance of television in the lives of children, Taka's (161) exploratory work on a developmental approach to children's grasp of montage and visual symbolism and to their general cognitive and emotional responsiveness to motion pictures is deserving of mention. For instance, he finds that, while cognitive accuracy increased with age, apt awareness of the affective tone of scenes was not much different between third-year primary and second-year junior high school students. The research is also suggestive of ways to improve the communicative adequacy of sequential visual presentations directed to children.

Taking the opportunity afforded by the coming of the Tokyo Olympics, Fujitake (57) examined the responsiveness of the Japanese people to this new and unusual event. His results suggest the existence of three stages spread over a period of months in the process of psychological involvement in the Olympics: (a) awareness of the forthcoming event, coupled with sketchy knowledge and a slight uplift of interest; (b) then the acquisition of

more detailed knowledge, an upsurge of interest, and the arousal of a weak emotional involvement; and (c) the condensing of heightened emotional involvement around the event which now takes on more central significance. These stages reflect changes in the density and substance of media coverage over time. For the vast majority of people, the Olympics was the reality created by television in the form of a public drama that took place in television time and gave central focus to the competitive efforts of Japanese athletes.

Race as a factor.—Although opinion surveys on social and political topics customarily provide information on the views of groups separated on race, religion, or ethnic background, there has been relatively little comparable attention to such group differences in the uses of the media. On the basis of a nationwide study, Bogart (17) reports that demographic factors were related to media usage among Negroes in the same manner as among whites. However, Negroes were less exposed to all media, including television, with proportionately four times as many not exposed "yesterday" to any of the media. Undoubtedly, group differences in education and income were primary factors contributing to differential media usage (69, 71). But, as Lyle (104) avers in regard to Los Angeles newspapers, distrust of the 'white' media may also be involved.

Carey (27) reports a study of black-white differences in preferences for television programs. While the ranking of programs according to reported viewing shows significant interracial correlations, the common cultural taste implied by this accounts for only approximately 25 percent of the variance and seems to relate more to agreement on nonpreferred than on preferred programs. Only three of the programs in the top ten for each group are the same, and sharp Negro-white differences appear for others in this set. Based on the pattern of Negro preferences, Carey puts forward cautiously the descriptive generalization that, from the available programs, blacks favored those that focused on individual action in situations of dramatic tension or conflict and avoided white family-type situation comedies. He notes the relevance of this pattern to cultural-situational factors of Negro life. More recent findings suggestive of a similar preferential pattern among Negro teenagers are provided by Greenberg & Dominick (70).

The potential utility of such data as evidence of the cultural significance of changing life experiences and circumstances of Negroes and for black-white cultural accommodations suggests the value of longitudinal studies. In fact, Carey (27) mentions that in a survey a year later the common variance in program rankings approached 50 percent. Current research by Greenberg and associates (69, 70) points to the significance of economic status and age as factors in such comparisons. Among low-income adults, the rank-order correlation between Negro and white preferences for 12 television programs was .72; but the correlation across income levels was near zero (69). (The results are similar in regard to preference for different

sections of the daily newspaper.) However, among teenagers (70), both race and economic status were influential associates of program preferences, acceptance of television programs as accurate depictions of reality, and use of the programs as a 'school of life' and for vicarious excitement; the order for the latter two was: low-income Negro > low-income white > middle-income white.

Gerson (62) examined the differences between Negro and white adolescents in their use of the media as a source and a confirmer of ideas relating to dating behavior. He found that among males Negroes were more likely to acknowledge using the media for both purposes, whereas among females there was no racial difference. In the absence of a more probing analysis and of data on particular media used and on types of behavior learned or reinforced, the proper interpretation to be given to these and other findings is uncertain.

NEGROES AND THE MEDIA

In general, there has always been a dearth of attention to the contents and functions of media vehicles designed for specific ethnic, racial, or religious groups. Instead, research has focused almost exclusively on the media for the general public. In regard to the contemporary scene, although the black social revolution has been a major theme for the past decade, there has been relatively little research on the Negro press or other media directed at black audiences.

Negro press.—Some relevant data are reported by Roshco (137) and Lyle (104). Both note that the Negro press has experienced a decline in circulation in recent decades. The primary reasons are probably reducible to the relationship between the traditional nature of the Negro press and the national newsworthiness plus the achieved though limited successes of the Negro social revolution. Before black activities became generally and continuously of interest to the general public, Negroes had to turn to the black press for such news and commentary. But within the last decade the general mass media have begun to devote news space, features, and analyses to racial relations and Negro protests. For instance, the demise in 1966 after almost 50 years of the Associated Negro Press news service has been attributed to the servicing of Negro papers by the major national wire services, once they began continuous coverage of Negro news (7).

In addition to the impact of reduced functional utility, the Negro press suffered from a loss of a major core of readers when the Negro middle class began to move from the ghettos to the suburbs as their economic condition and residential options improved (137). On the basis of the differential attentiveness of lower and middle-class Negroes to the same ads in a black and a major newspaper, Petroff (123) arrives at a comparable conjecture. Some of Lyle's (104) data are also supportive of this inference.

However, the preceding should not be taken to imply the imminent

demise of the Negro press. Despite the attention given to black problems and activities, the focus of the daily media is still on major news stories and not on the continuing run of events and diverse interests of Negroes nor on local community activities and personalities. The major defect from the Negro point of view is the failure of the dominant media to provide a Negro perspective on relevant news stories (66, 87, 121). This is the strength of the Negro press, embracing as it does news, editorials, columns of commentary and analysis by black journalists and spokesmen, and letters to the editor from ordinary people.

Media portrayals.—The evidence of current research on the depictions of Negroes in advertising accords with their changed and changing status in American society. Using the same magazines as Shuey, King & Griffith (151) did approximately 18 years earlier, Cox (38) finds that over this period there was a steep increase from 6 percent to 71 percent in depictions of Negroes as skilled or white-collar workers. Kassarian (85) also confirms this general pattern of change. Colle (35) notes that since 1960 Negroes have become more visible in movies and television and are portrayed in many different roles. Although the new media image can encourage Negro aspirations, pride, and dignity, he feels that the most significant outcome will be on the greater acceptance by whites of the idea that Negroes belong in the mainstream of American life.

In a comparison of content in selected general and Negro magazines, Barcus & Levin (6) analyzed the role distance between characters in short story fiction. The general patterns were quite similar in both types of magazines; essentially, the distance between people of the same race was closer than between people of different races. But in the general magazines, black-white contacts were tangential to the main story line, and the stories ignored racial conflict and the civil rights struggle; whereas, in the Negro magazines, the fiction drew heavily on contemporary problems of black-white relations. It is quite likely that the different purposes and audiences of the magazines influenced the editors' judgments of suitable stories. A similar comment is applicable to the evidence (139) that during 1962-63 *Ebony* magazine featured nonviolent newsphotos of civil rights conflicts, while *Life* magazine emphasized the violent; also a higher percentage of *Ebony's* photos displayed mixed racial associations.

VIOLENCE AND THE MEDIA

News media and civil disorders.—The Kerner report (87) on the 1967 ghetto disorders neither absolved nor charged the media, particularly television, with exacerbating the riots. Although the media's influence was not considered decisive, the report did criticize the coverage on several grounds. The major inadequacies were the media's failure to relate the riots to the deprivational and corrosive background of ghetto life, to provide access to a black-oriented perspective on the events, and to undercut the

wide impression of a primary black-white confrontation. Also, the media were charged with failure to give adequate attention to the representativeness and mood of the picture being transmitted. Other commentators (5, 73, 78, 121) note the proclivity of the wire services toward exaggeration, particularly in the early reports of disorders.

Television entertainment.—Based on surveys of related literature, several contributors to a governmental report on violence and the media (94) have been led to a firm endorsement of the position that televised violence in entertainment programs has demonstrably adverse effects on behavior, attitudes, and values; their certainty of the charge of guilty seems only a shade lower than Wertham's (176). However, the judgment appears to be founded primarily on moot extrapolations from adduced evidence whose conclusiveness and intimate relevance are open to reasonable question (74, 175). Also, despite some acknowledgment, the wider societal and historical context pertinent to an understanding and evaluation of current media effects is given little weight (39, 40, 42, 56, 63, 67).

A study exemplifying the difficulty of easy interpretations of media effects is briefly reported by Harper, Muro & Himmelweit (75). Based on the factor analysis of rated preferences for 34 television programs by British schoolchildren, 11 program types were found. One constituted a preference area which contained Westerns and two animal programs, but not one crime or detective program. This suggested to the authors that 10 to 11-year-olds may react more to the outdoor life and the riding than to the violence of the Westerns. Also, pop music was the favorite type of program, whereas 10 years before an adult detective program was the favorite. According to the authors, both preferences may reflect popular fashion rather than psychological needs.

Also pertinent to the interpretive problem are Lovibond's (103) correlational data demonstrating a significant association between Australian schoolchildren's approval of statements from war and crime comic books emphasizing toughness and force and their exposure to crime and violence programs on television. But even before the advent of television, a significant association existed between approval of such statements and exposure to the popular media of the time. The author notes that a cause-effect relationship cannot be deduced from the data; at best, only conjectures of mutual interaction and common predispositions fostered by other factors can be made.

Experimental research on media portrayals of violence.—A major theme in experimental research on the effects of vicarious aggressive experiences derives from elaborations of a frustration-aggression hypothesis. The relevant core of Berkowitz's formulation (13) holds that anger arousal increases aggressive tendencies but that the likelihood and intensity of their expression is a function of the degree of anger, the strength of past learning to be

aggressive, and the presence of situational cues inhibiting or facilitating aggression against the anger arouser. In a study by Green & Bekowitz (60), subjects were provoked by shocks delivered by the experimenter's confederate to signify a moderately poor evaluation of their solutions to a problem. Subjects then watched either a track or bloody fight film, and, following this, shocked the confederate as a way of evaluating his solution to a similar problem. In general, only when there was some similarity between the names of the confederate and the beaten fighter, did viewers of the boxing film give more shocks than viewers of the track film. The significance of the name linkage in connection with exposure to the justified movie beating was also found in other research (14, 61) involving shock or insult as the principal instigating condition and employing several procedural variations. The general data patterns are construed as evidence that the filmed depiction of a person who resembles even tenuously an actual aggressor lowers inhibitions against the expression of counter-aggression. It is significant that the film effect is *not* attributed to a heightening of aggressive tendencies, and the necessary conditions are prior aggression arousal, belief in the justice of the filmed aggression, and an association between the film victim and the viewer's own aggressor.

However, despite the absence of the latter conditions, Hartmann (77), using a 2-minute film the last minute of which showed a fight between two boys who had previously engaged in basketball play, obtains some evidence that even among noninsulted subjects the fight sequence can lead to higher evaluation-shock. Whether the result is due to the use of delinquents as subjects or whether aggressiveness was heightened or inhibitions reduced is unknown. Also, only among boys who had been insulted by the other participant and whose records of delinquent offenses were above the average for the sample was the movie version that focused on the victim in the fight more effective than the one that focused on the attacker. Among all other subgroups, the reverse was the trend.

Neither these nor previous findings nor their conceptualization by Berkowitz lend support to a catharsis hypothesis. However, Bramel, Taub & Blum (21) report some evidence in support of a catharsis assumption that hostile feelings toward an aggressor would be mollified by the satisfying experience of merely hearing him suffer. Goranson (65) mentions unpublished research that utilized a bloody fight film which ended with the beaten fighter either physically devastated and dying or in good shape and achieving success later on. Fewer shocks were given by the subjects to their aggressor after viewing the former film than after viewing the latter one. Similar results were obtained when the distressing ending was not an aftermath of the fight. However, heightened concern over the possible hurtful effects of the shocks rather than a cathartic reduction in aggressive tendencies is more likely the meaning of these results.

In all of the literature relevant to media portrayals of violence, no experimental manipulations have been carried out in a reasonably naturalistic

setting on the effects of a long run of exposures to the usual kinds of televised entertainment programs. Now Feshbach (54) has completed an elaborate study of the kind needed. However, in the absence of a full report, particular caution must be exercised in considering his most unexpected findings. Nevertheless, boys from 8-18 years old assigned to a heavy diet of televised violence for 6 weeks showed reduced aggressiveness in their ordinary behavior toward peers and in their values, compared to boys given a diet of nonviolent television entertainment. But aggression in story-making themes was positively related to degree of exposure to television violence. Feshbach suggests that the usual aggressive content of television programs may provide cognitive support to "bind" or regulate the behavioral impulses of aggressively disposed boys. It is of interest that such an interpretation does not reflect the usual perspective of a simple catharsis hypothesis.

If anger is the energetic force for emotionally based aggression, then its displacement as the dominant feeling should lower the strength of aggressive tendencies. On this ground if no other, a satisfying humorous experience is often thought of as having such an interfering effect. Research by Singer (155), Dworkin & Efran (50), and Landy & Mettee (90) support this inference and provide some suggestive evidence of the particular efficacy of hostile humor. Evidence of this sort makes more pertinent and suggestive the findings that cartoons in two national magazines frequently display hostile or violent behavior (52) and that aggressive humor tends to be rated funnier than nonaggressive (64, 156), except when inhibitions against aggressive expression are aroused or the humor's aggressiveness is too salient.

In the main, the preceding research has been concerned with the effects of vicarious experiences on the expression of aggressive behavior toward a personal frustrator, with the form of the aggression being unrelated to the depicted behavior. In contrast, studies of the modeling effects of observational learning focus primarily on the imitation of specific and novel aggressive responses made by adult models toward inanimate play objects. The observational and test settings are very similar; and the subjects are primarily young children of both sexes. Since the modeled responses are most unlikely to be found in ordinary play with the test toys, the research has consistently demonstrated some increase in the children's use of the model's behavior. Hence, recent experimentation has tended to focus on the effects of induced or stable dispositions or of verbal evaluations of the model's aggressive play.

Walters & Willows (173) sought to compare the modeling effect on normal and emotionally disturbed boys of a film depicting a female adult model who behaved aggressively or nonaggressively towards a number of toys and accompanied her behavior with suitable comments. Disturbed boys showed as many aggressive responses as did the nondisturbed boys but almost significantly fewer nonaggressive responses. Essentially, the boys drew on the film model's behavior during the 6-minute period they were told to "have fun" in the toy room. But the preponderance of responses were

nonaggressive ones; and the aggressive model raised the baseline average from zero to only one aggressive response.

Nelson, Gelfand & Hartmann (116) found that the aggressive play behavior of a live adult male had no effect on the play behavior of boys but brought out more aggressive play in girls, presumably through a lowering of inhibitions. Although a prior induction of task failure increased aggressive play, the effect of the aggressive model was not enhanced by this frustrating experience. However, Kuhn, Madsen & Becker (89) reported an effect of prior frustration on responses to an aggressive model. But the direction was contrary to expectation since the frustrating experience diminished the aggressive model's effectiveness.

Using a film showing a white male adult aggressing against a bobo doll, Thelen & Soltz (164) tested a predominantly Negro sample of boys entered in a summer Head Start program. Contrary to expectation, aggressive play behavior was greater when the acts of the film model were not followed by the reinforcing, congratulatory verbalizations of a recorded male voice. A follow-up experiment with white children attending a university-related school found the verbalization version to be slightly better than the no-verbalization one. Since the two samples undoubtedly differed in a number of relevant characteristics other than race, the particular significance of a white model can only be entertained as one possible factor in the different data outcomes.

Hicks (79) examined the effectiveness of having a male adult stranger make approving or criticizing or no comments about the filmed aggressive play of another male adult. Only when the commenting adult was also present though silent in the separate test room did his earlier comments produce a differential effect on the play behavior of young children. Their imitation of the aggressive acts of the film model increased when his prior comments had been approving and decreased when they had been disapproving. Somewhat similar results are reported by Rosekrans & Hartup (136), who used a live adult model and had the experimenter make the commentary on the model's acts.

The preceding set of studies seems to suggest that, except for a simple effect of sex, dispositional variables do not function as expected, that the modeling effects are more likely due to a lowering of play inhibitions or a learning of novel ways of handling toys rather than an increase in aggressive tendencies, and that verbal commentary associated with the model's behavior can have an effect under certain conditions. Absent, unfortunately, is an understanding of the subjective meaning to the children of the observation experiences and their play responses.

DIFFUSION AND NATIONAL DEVELOPMENT

Diffusion of innovations.—Research on the diffusion of innovations, particularly in agricultural practices, has reached voluminous proportions, with as many studies published in the period 1962–67 as in the preceding

34 years (132). [See Rogers (131) for an extensive bibliography of references.] However, despite the considerable efforts devoted to diffusion research, a number of the significant contributors to the literature have expressed critical concern over the state of current theory and methodology (26, 37).

Since adoption of an agricultural innovation involves risk-taking, it is not surprising that early adopters are characterized by relatively higher economic resources (130). But so should the availability of relevant psychological resources foster early adoption. In confirmation, Bohlen (19) finds evidence that early adopters more than later ones exhibit the conceptual ability to evaluate and use abstract and technical information and thereby arrive at an early decision. Also, they believe themselves "masters of their fates" and able to cope with unforeseen contingencies. In traditional areas of the world, the obverse of this confidence in one's ability to cope and, by personal effort, to change a situation appears in the form of an attitude of fatalism, which has often been considered a significant obstacle to change. For instance, in India it is found to inhibit acceptance of new agricultural practices (31). But in regard to Colombian villagers, Rogers (133) sees it more as a rationalization for failure to induce change than as an insurmountable barrier to modernization in farming. Examining many case studies, Niehoff & Anderson (117) concur that fatalism from whatever source is not by itself sufficient to prevent change in farming or health practices or to cause the failure of development projects, as long as the formal change agents are skillful in technique and utilize whatever positive factors and incentives exist in the situation. However, they are more concerned with a "project fatalism" bred of unsatisfactory experiences following the adoption of recommended practices or the completion of a development project, which they believe is increasing and will make future change more difficult. This concern is also expressed by Dube (48), who notes that change agents may fail in India because of villagers' poor experiences with past innovations.

In regard to the adoption of new farming practices in India, Sawhney (142) finds that for all stages of the adoption process, personal nonlocal sources (essentially extension agents) are named far more frequently than personal local sources (friends and neighbors); and both types of personal sources are named more frequently than the media which reach a high point of mentions of approximately 12 percent as a source of first knowledge or a source for additional information during the interest stage. Personal sources, particularly nonlocal ones, were of greatest importance during the evaluation, trial, and adoption stages. Sawhney suggests that the discrepancy with Rogers' generalization (130) that cosmopolite sources are most important at the awareness stage and localite sources at the evaluation stage may be due to the relatively low technical competence of Indian farmers and their limited experience with new practices. In his own research in Columbia, Rogers (133) finds that personal local sources (farmers) are mentioned more frequently than personal nonlocal ones (extension agents and farm store personnel) at all stages of the adoption process for a new farm practice,

except at the trial stage where they are equivalent. He suggests that local sources affect attitude toward and use of the innovation, whereas nonlocal ones make their contribution by providing technical information. Sawhney's suggestion about relative competence of local farmers may account for the difference between his and Rogers' current data, and implies the need to assess and take into account the relative credibility of different sources in the adoption process, a factor considered by Wolpert (178) in his computer simulation of agricultural diffusion in Sweden and proposed by others (101, 132). Also, despite the reverse order of significance of types of personal sources, both studies support Rogers' proposal that the personal local/non-local (or cosmopolite) classification of sources may be more useful in less modern regions than the personal/impersonal (or media) differentiation derived from research in highly industrialized countries. The data of Parker & Paisley (122) on adult information seeking in the US would be consistent with this suggestion, since the mass media are the dominant sources for diverse kinds of information. It is principally when the information is to be applied concretely or on matters of high personal involvement that interpersonal sources challenge the primacy of the media.

Opinion leadership.—There is continued interest in the role of opinion leadership in the diffusion of information, ideas, and practices. Using the customary conception, Rogers (133) and Rao (126) provide data essentially confirming past findings on leader attributes and on the significance of social context and modernization influences on leadership. However, others have begun to use a more differentiated view of the giving-receiving activities involved in informational and influence transactions by explicitly utilizing the perspective that people can function as both givers and receivers (152, 153).

Wright & Cantor (179) separated graduate students into "opinion seekers" and "opinion avoiders," based essentially on self-reported frequency of participation in discussions on four topics, one being foreign affairs. Also, by self-designation, the students were separated into opinion leaders and nonleaders on each topic. While there was overlap between the two categorizations, it was not complete. (However, a similar self-report question does link the two categorizations.) Interest in foreign affairs and relevant print media usage was greater for opinion seekers than for avoiders; but opinion seekers, whether or not they were categorized as opinion leaders, were comparable in interest and in print media usage, whereas leaders who were not also seekers were somewhat less interested and exposed to relevant communications. On these data the authors propose that past research showing heavier communication exposure of opinion leaders may have reflected the active opinion-seeking characteristic associated with leadership rather than leadership per se. Consistent is the finding of Troidahl & Van Dam (167) that both members of discussion dyads were comparable in media usage and information level, whether they were seekers or givers of

information, whereas those not participating in discussions on the topic were lower in these characteristics. Hence, designated opinion leaders may not pass on information as much as compare views and share knowledge with others of comparable knowledge and involvement in the topic. A similar possibility is expressed by other researchers in regard to agricultural diffusion (19, 101) and the initial purchase of a new consumer product (4).

National development.—In the modernization of nations, the media of mass communication play a central role. But for successful national development they must not only seek to contribute with other societal institutions to the channeling of aroused wants and increased economic resources into a pattern of deferred gratification but also to provide a realistic assessment of what is possible now (47, 99, 126). Another not well-realized function is to link knowledge resources to policy makers and planners and not just to the general populace (48, 96). While social scientists have accepted this perspective of the centrality of mass communication in the modernization process, developing countries under non-Communist rule have usually failed to view the media's fundamental significance as a 'power' resource and to secure its growth in the overall planning for national development (47, 51, 125).

Although the preceding remarks apply a broad brush to the general significance of the media, they do not reveal how the means of mass communication fit specifically into an analytic model of national development. While no single model is agreed upon by researchers, the following abbreviated and gross one would probably be found acceptable as a starting point (99, 107, 147): urbanization, taken as an index of industrial development and economic adequacy → the development of educational resources, and thereby literacy and significant cognitive and attitudinal effects of education → the development, extension, and use of mass communication → a variety of modernization outcomes. This part-model should be understood not merely as a description but also as a plan of action; for not only does urbanization not lead directly to modernization, instead requiring the mediating contributions of the educational system and the media, but also the potential contributions of the media presuppose the establishment of a functioning and effective educational system. Furthermore, while the arrows indicate the main directional effects, the system is actually looped embracing mutual interactions between the parts. Although Lerner (98) had proposed certain quantitative values for earlier elements of the sequence as take-off points for the rapid expansion of following ones, it is unlikely that these posits will survive changing world conditions or the likelihood of regional differences within the overall pattern set out by the model (147).

[A significant report by Guthrie (72a) underscores the constraining influence on innovational behavior in a developing country of existing social norms and reinforcement contingencies, despite the acceptance of modern-oriented attitudes and values.]

In confirmation of the model's general pattern, Chu (33) found that newspaper readership was an important attribute of opinion leaders, but primarily among the better educated who are high in economic and social status. A similar but weaker pattern was observed for leadership to be more common among listeners to radio information than among entertainment listeners or nonlisteners, with the latter two subgroups being comparable. In another study, Chu (32) found that only among those of high economic standing was deferred gratification behavior more likely to be exhibited by respondents who read business news rather than local or domestic or crime news, or who listened to information programs on radio rather than to entertainment programs or to no programs at all. Also, in conformity with the general model, when relevant newspaper reading was statistically controlled, neither education nor economic standing was significantly related to deferred gratification behavior. But newspaper-reading remained correlated with deferred gratification when the other factors were held constant.

In a study of a southern Appalachian community, Donohew (43) found that, although general radio and television exposure was not related to an index of receptiveness to change, scores on the index were higher for those exposed to news and public affairs programs than for those exposed to radio or television. However, in further research on the same population, Donohew & Singh (45) reported that those more in contact with the wider environment, directly through trips or vicariously through the media, were lower in the adoption of practices recommended by local poverty agencies than were those more isolated from the outside world. Perhaps the practices were more congruent with a local, traditional orientation or with minimal change in style of living than with an outward-looking, change-aspiring orientation.

Rogers (133) finds that media exposure was not as critical as other attributes—including political knowledge, innovativeness, and social status—as a predictor of agricultural opinion leadership in Colombia. Perhaps this is due to the inadequacy of too gross a measure of exposure, particularly since political knowledge is so strong a predictor. Also implying confirmation of the model are the results of Waisanen & Duriak (172) that in Costa Rica the use of various print media or of television bears a stronger relation to a modern view of the world, even with education and income controlled, than does radio listening. They suggest that this result may have occurred because radio listening is more consummatory and is available to poorer and more rural people. Support for their conjecture is found in a study by Edwards (53), who observed that influentials in Costa Rica were more likely than others to use television and print media, particularly cosmopolitan publications. Also consistent are the findings of McLeod, Rush & Friederich (109) that in Ecuador total media exposure was only weakly related to general knowledge of political and public affairs, whereas reading 'hard' news in the print media was more strongly related to such knowledge. The latter relationship held even when socio-economic status was statistically controlled. Similarly, in a US study (111), usage of cosmopolitan print media

and knowledge of international affairs were correlated even when education was controlled; however, in this modern urban setting, the relationship was stronger among those who had not attended college than among those who had.

Assertions that broadcast or print media have little impact in a country because there are few radio or television sets or because the literacy rate is low do not usually take into account multiple or out-loud reading of individual copies. For example, in India it is common for one paper to be read by several people in the villages and also read aloud to many more who are illiterate (51, 114, 126). In fact, Eapen (51) reports that about 40 percent of the illiterates in his sample had received news of the India-Pakistan conflict in this way. Hence, circulation per some amount of population is an inadequate index of the reach of newspapers or their importance in a press-poor country.

In regard to broadcast media, the significance of audience figures as distinct from sets is well known in industrialized countries. The same should apply to less modern areas since group listening or watching is usual, as in West Bengal (22), even when the multiplying effect of organized group exposure as in radio or television forums (55, 170, 175) is not a factor. Hence, the social impact of the media, particularly in the less modernized nations, is not adequately assessed by a ratio of physical units to population.

The significance of the personal element in the diffusion of news and innovations is evident in modern nations as well as in developing countries. But it is in the latter with their strong traditions of oral communication that face-to-face contacts become dominant in the mass communication process. Even in the more modern village of the two studied by Rao in India (126), all of the respondents referred to other people as sources of various items of general information, with less than half referring to newspapers and even fewer naming radio. In the West Bengal region, while word of mouth was the principal channel of news for illiterates, it was a significant source even for those with a primary school education (22). Dube (48) found that 83 percent of the people in 198 villages in India were aware of the Chinese-Indian border conflict shortly after the Chinese attacks occurred. Even in Indian states distant from the Himalayan region, awareness was almost as high as in nearby states. The primary first channel of news of the conflict named by over 40 percent of the sample was personal transmission, particularly by friends and neighbors; radio and television were slightly behind. In regard to sources for more ordinary matters of public affairs, 88 percent in another survey named some personal source and only 12 percent designated the mass media. However, it is likely that many of the personal sources obtained their information from the radio or newspapers. It is on the basis of such evidence of the wide and rapid diffusion of news by personal channels that Dube (48) considers the purported informational isolation of India's villages to be more a myth than a reality.

While the essence of topical news can spread rapidly and widely by word

of mouth, knowledge of the national interpretation and evaluation of information requires contact with the mass media. Lane's (91) study in rural northeast Brazil points to the significance of this normative function of the media. The isolation of the peasantry from national communications through the media, and not simply rural unrest or contacts with political organizers, was the significant influence on their favorable regard for radical political leaders.

For fuller comprehension and more refined analysis of the role of personal channels in the mass communication process, research is needed on the normal pattern of conversational links between people and on the normal frequency and substance of such conversations. That is, information should be obtained on who generally speaks to whom, how often, and about what (101). Despite the multitude of diffusion studies and the key significance assigned to personal contact, there is a dearth of normative knowledge on such matters. Research on opinion leadership is strangely deficient in this regard, with only the most limited data gathered through the self-report responses used to identify those who function as opinion or information resources for others. A mapping of normal conversational contacts would be of particular relevance for less modern countries, in view of their greater dependence on personal channels for the diffusion of news and ideas. For instance, in a modernizing village in India, Rao (126) finds that the patterns of social intercourse are diffuse, permitting a vertical flow of information; whereas in a more traditional village, personal relations are more restricted to socially separated segments of the populace, making vertical diffusion less likely to occur and impeding the spread of modernizing ideas from the elite who are likely to be the ones in contact with the outside world (cf. 47, 48). Nakajima (115) notes that, while conversation is a significant channel for the spread of information in Pakistan, diffusion occurs horizontally within restricted social strata. Hence, it is important that at least some members of the lower strata come into contact with the media in order that nation-building information be spread throughout the general populace.

Diffusion of news in the US.—The following are the major questions addressed in recent American research on the diffusion of significant news events: (a) what are the sources that provide first awareness of an event; and (b) what are the characteristics of people who are aware? Essentially, the mass media are the predominant first sources; excepting President Kennedy's assassination, personal channels provide first awareness for relatively few people (1, 3, 25, 100, 111, 119). Although personal channels increase in importance as first sources with an increase in the importance of the event (25, 100), the magnitude of the effect is not large nor does it raise personal channels to a major source of first awareness. As might be expected, knowledge of an event is positively related to educational level (1, 3, 25, 100, 111) and, to some extent, to the event's purported relevance to personal interests (1, 100). Also influential are normal media preferences (119).

However, time of day and day of the week (e.g. Sunday) when news of the event first broke affect source of first awareness (25, 119), since certain media may not be available or may be unduly favored because of what people are doing or where they are. These uncontrollable factors may account for much of the reported variation within the general pattern of results.

POLITICAL CAMPAIGNS

Effects on voting.—Despite the beliefs and assumptions of people in and out of politics, past evidence does not establish the power of the media to persuade or convert significant numbers of people to new political preferences during the period of an election campaign (175). The primary outcomes of campaigning appear to be the reinforcement of existent voting intentions, the strengthening of weakened preferences, the solidification of precampaign shifts that resulted from the effects of interelection events, and only modest effects on knowledge or perception of candidate or party positions. Evidence from British research on the 1964 general election (16) confirms in broad outline these general conclusions (cf. 10). The formal 5-week campaign produced a slight improvement in knowledge of party positions, left unchanged general appraisals of the most important issues facing the country, and had no significant effect on the vote for the dominant Labour and Conservative Parties. However, the campaign did increase the vote for the small and much less well-known Liberal Party. Free television time, particularly for party broadcasts, was the key to the gain by the inveterately underpublicized Liberal Party, for television was almost its sole nationwide communication resource (135).

In view of the absence of campaign effects on the overall votes achieved by the Labour and Conservative Parties, Blumler & McQuail (16) attribute Labour's victory to the impact of events occurring between election day 1959 and the formal opening of the 1964 campaign. However, both major parties had begun their propaganda efforts in mid-May 1963, approximately a year before the then expected date for the election (135). Hence, what changes in vote intentions were produced or what latent shifts were crystallized by the year-long effort by both parties were neither assessed nor taken into account.

Nevertheless, in view of the dominant role of television in the enormous and increasing costs of US election campaigns, it is of interest that the Liberal Party gains were achieved with only three-fifths as much party broadcast time and much less general publicity on television than that provided the other two parties. Hence, caution is needed against accepting any simple assumption about the significance of gross broadcast time for campaign effectiveness or against confusing the campaign itself with all its complexities with its reflection in the media (76, 135). In confirmation are data reported by Alexander (2) revealing the absence of a relationship between the total amount of money spent on radio and television programs of 5 minutes or more and either election outcomes in US senatorial or

gubernatorial elections in 1960 and 1962 or the state-by-state popular or electoral vote in the 1960 presidential election.

In regard to the purported influence on voting of editorial endorsements by the press, the few recent studies available are based on gross and limited correlational analyses. The evidence suggests at best a minor impact limited to voting situations of little general interest and importance and where there are few other informational influences operating (72, 83, 106).

Candidate images.—By its very nature, television brings the candidate face to face with a vast at-home public and fixes the viewer's attention on the candidate's appearance, manner, and style of personal presentation. Hence, there is concern that a manipulated reality will be produced by media consultants who are paid to manufacture an "image candidate" (180).

A summary of the limited past research on the effects of television on candidate images would permit the following inferences: (a) the less well known the candidate, the more television can modify viewers' perceptions of his characteristics (36, 88); (b) television and radio broadcasts of the same address by a candidate can yield differing perceptions of his characteristics (124, 180); (c) viewers of differing political attitudes take away similar perceptions of some personal attributes of the candidate and differing perceptions of others (28, 108, 154, 163); and (d) changes in candidate image are not clearly related nor central to voting decisions (175). Unfortunately, research has not been frequent and systematic enough to determine what clusters of characteristics are more likely to be affected and under what conditions, and what characteristics are most central for voting decisions and under what conditions.

The research by Blumler & McQuail (16) provides confirmation of some of the preceding inferences and extends our knowledge of television's effects on perceptions of candidates. Their respondents not only ranked attributes such as "straightforward," "sincere," and "confident" high among traits desirable for someone who is to run the country, but also judged them to be traits that television is most able to reveal. However, other important attributes, such as "strong" and "hardworking," were considered unlikely to be revealed by television election broadcasts. But in regard to actual changes in perceptions of the leaders of the major parties, the campaign was found to have influenced ratings primarily on a cluster of characteristics connoting personal strength or toughness (cf. 28). Supporters of different parties changed their judgments in the same way and to the same extent, even though two of the leaders improved in this quality and one declined. Hence, changes in leader images were unrelated to voting.

Of particular significance is the authors' assertion that the most difficult of television's effects to assess was its effects on leader images. This was due to the difficulty of finding a means to extract from gross exposure that part relevant to the leaders per se. Nevertheless, extent of exposure was not consistently related to changes in leader images. Although changes in attitude

to a party and to its leaders covaried, consistent party supporters were more favorable to their party than to its leader. In general, the authors note that perception of the leader's qualities are no more amenable to change than is attitude to the party and that there is little support for the presumption of the image-building power of the media.

Also contrary to any gross presumption of remarkable media effectiveness in image building in political campaigns is Rose's (135) report that, despite more than 6 months of advertising emphasis on the leaders of the Labour and Conservative Parties before the 1964 election, popular approval of both declined and to a greater extent than did approval of their respective parties. A comparable decline in personal popularity for Johnson and Goldwater during the 1964 presidential campaign has also been reported (12). Probably such effects result from the arousal of increasingly partisan associations by the usual intensive political advertising and party propaganda during an election campaign.

Selective exposure.—In studies of political behavior, selective exposure refers to differential attentiveness to political communications as a result of congruency between the source (candidate or party) or between the anticipated attitudinal significance of the message and the attitudes of potential receivers toward the source or the communication content. The general form of the relationship can be stated as: the greater the congruency, the greater the likelihood of exposure, whether by active seeking out or by usual attentiveness to customary channels of communication. The sum of past evidence from voting studies suggests that the selectivity which occurs is unimpressive in magnitude and insufficient to keep people uninformed of the major arguments and positions of opposing candidates and parties (148, 149, 175; cf. 86, 113). Also, despite a slightly favorable bias in some aspects of the news treatment of editorially endorsed candidates, the general press tends currently toward equitable and ample treatment of the major contending candidates (9, 159).

In confirmation of research on the 1959 election in Britain (165), Blumler & McQuail found modest selectivity in their study of the 1964 general election. While supporters of one of the three major parties watched more broadcasts by their preferred party than by either of the others, they nevertheless did watch more of the others combined than of their own. Similar results are reported for the 1966 general election by Harrison (76), who also noted that the political composition of the audiences did not vary over the broadcasts by the different parties. However, simultaneous transmission on all channels, hence eliminating viewing options, may account for some part of audience comparability over party broadcasts.

The rich data of Blumler & McQuail (16) reveal that only about one-half of the people watching party broadcasts on television do so for politically relevant reasons. In fact, extent of ordinary exposure to television is the strongest determinant of exposure to political television; degree of interest in the campaign plays a significant role in exposure to political material on

television, primarily when customary set usage is moderate or light. Significant selective exposure was found mainly among the roughly one out of four people in the total sample who stated that their principal motive for watching political television was a desire for reinforcement of their views and "ammunition" to support their views in discussions with others. These respondents were more stable in their vote decisions, more partisan in their attitudes toward their party, and also more likely than others to assert that, when they avoided political broadcasts, it was because their minds were already made up. Hence, it was those least likely to be changed by exposure who tended to avoid party broadcasts. Interestingly, they were also more likely to be categorized by the authors as opinion leaders.

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DEVELOPMENTAL PSYCHOLOGY¹

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Developmental psychology is passing through a period of unprecedented expansion. Research activity in this field continues to increase and all signs point to further expansion in the years ahead. The American Psychological Association began publication of *Developmental Psychology* (772 pages), the *Journal of Experimental Child Psychology* published 400 extra pages in 1969, and lesser increments were marked in several other publications. Slightly fewer developmental studies now appear in some of the older journals, but that relief is minimal compared to the pressure that is mounting on the newer outlets.

The results of this activity are variable. There is variation in topic, theory, and method, but there is also unevenness in quality. Under these conditions, however, two uncommonly good publications have appeared: *Principles of Perceptual Learning and Development* by Eleanor J. Gibson (138), and *Attachment and Loss* by John Bowlby (36). Both of these works have important implications for developmental psychology, and for this reason we turn our attention immediately to them.

Perception, learning, and development.—The theory of perceptual learning proposed by Gibson contrasts sharply with traditional stimulus-response formulations. It begins by assuming that the stimulation available to the organism is rich in many different types of information. Thus, perceptual learning is not conceptualized as supplementing bare sensations or associating responses with them but, rather, as the differentiation and extraction of information present in the stimulation itself. What is learned are distinctive features (properties that distinguish the members of a set of objects) and the invariants of events, as well as higher-order structures of both. The problem of perception, in a sense, is the overabundance of information contained in the stimulation reaching the organism, and the mechanism of learning is believed to be *selection* through filtering and abstraction.

According to this view, perception is an active, adaptive, self-regulating process. Perceptual change is directed by an internal reduction of uncertainty that results in increased efficiency with which information is processed. Processes of perceptual learning are thought to remain essentially the same throughout development, although certain age trends are hypothe-

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sized, e.g. a trend toward greater specificity in perception, greater optimization of attention, and an increasing economy in the processing of information.

This picture of the organism, which stresses active searching for information, exploration of the environment, and refinement of perceptual processes, has far-reaching implications. It replaces the associationist, passive view of the organism and assumes self-regulating, internally motivated processes of learning. The theory is centered on perception, but it applies to other behavior systems as well, and since much of developmental psychology has deep roots in associationist traditions, these new formulations should have profound effects on this field. Actually, a considerable amount of developmental research related to Gibson's viewpoint has accumulated over the years; studies of stimulus preference, orienting, and other aspects of attention are discussed elsewhere in this review. We wish to acknowledge here, however, the originality of this analysis of perceptual learning and its implications for theorizing about other aspects of behavioral development.

Attachment.—Bowlby's analysis of social attachments is a blend of psychoanalytic assumptions, contemporary ethology, evolutionary notions, and certain aspects of control systems theory. Basic to this analysis is the assumption that human social behavior emerges within the framework of natural selection; that is, attachments to specific social objects are presumed to possess unique adaptive significance since they ensure survival of both child and species. Bowlby conceptualizes the infant's attachment to his mother in terms of species-characteristic behavior systems which at birth are relatively independent of one another and which manifest themselves at different times during the early months of life (e.g. sucking, smiling, clinging, and crying). These behavior systems eventually become focussed on the mother, serving to bind the child to her, and her to him. One of the hallmarks of this theory is its conceptualization of attachment in terms of the actions of *both* child and mother.

In its emphasis on behavioral pre-programming, Bowlby's theory may be more ethological than psychoanalytic. Essentially, however, it represents an important step toward an integration of ethology and psychology and harbingers the development of a new scientific field—human ethology. This viewpoint is already reflected in numerous publications (e.g. Ainsworth 5), but its future influence should extend far beyond the specific problem of attachment. This book, like Eleanor Gibson's, is a lucid analysis of a much larger problem: the interaction between endowment and experience in behavior and development.

Scope of the review.—The size of the literature and its overlap with other fields (e.g. educational and clinical psychology) have forced us to restrict the remainder of our review. Following recent precedent, we have omitted

developmental studies of nonhuman species; most studies of childhood psychopathology, retardation, and treatment; studies of classroom behavior; and research on adult development and aging. Owing to space limitations and our own idiosyncracies, many studies have been omitted that would otherwise have qualified for review. Even so, it has not been possible to discuss each in detail and, as a consequence, our citations are exceedingly telescoped.

INFANCY

Sensory and response capacities.—Neonates respond to relatively subtle changes in stimulation, and the behavioral repertoire is quite malleable during the first days and weeks of life. New data confirm that the perceptual capacities of the newborn are rather well developed. For example, the form of psychophysiological responses to flashing lights does not differ between newborns and adults even though amplitudes and latencies are not the same (Lodge et al 222). Responses to flashes of bluish white and orange light also indicate that both the color and achromatic visual systems are functional in the newborn, developing together at retinal and cortical levels. Studies of eye movements (382) showed that newborns respond to the movement of a light by shifting their gaze in the appropriate direction, and even though the two eyes do not usually converge upon a light stimulus, eye movements are well coordinated and a high frequency of conjugate movements occurs.

Evidence that the newborn has a much keener sense of smell than previously thought was provided by Rovee (307). Using a stabilimeter to measure activity, a power function was obtained between amount of movement and the concentration of vaporized alcohol to which the baby was exposed. This finding, as well as the results of Bond & Stevens (34), supports the hypothesis that the psychological power law reflects the inherent nature of sensory mechanisms rather than the effects of experience, as has sometimes been hypothesized.

Hutt, Lenard & Prechtl (171) stressed that "state" must be specified in neonatal research. This is not a new notion, but their data clearly show that the functional significance of state differs according to the behavior system and stimulus conditions involved. Consistent with this view, Lewis, Dodd & Harwitz (213), found that tactile stimulation produced *accelerations* in heart rate during sleep but *decelerations* during waking periods. Sex differences in the correlates of state were also reported (Korner 201). Spontaneous behaviors (e.g. startle responses, reflex smiling, and sucking bursts) were more frequent during deep sleep than during drowsing, but male babies showed more startle responses and females engaged more frequently in reflex smiling and bursts of sucking. Hutt et al believe that research dealing with the nonstimulated neonate should be accelerated and that priority should be given to experimentation with those kinds of stimulation that occur in the child's natural environment.

From this standpoint, two studies of infant crying (123, 124) possess questionable ecological validity since the response was elicited by snapping the infant's foot with a rubber band. Intensity of pain stimulation, however, was directly correlated with the lustiness of the baby's cry but not with its latency. Crying declined during the period from 2 to 6 hours after birth but was fully recovered by the 24th postnatal hour.

Research dealing with sucking is especially significant because this response is used frequently as a dependent variable in studies of infant perception and learning. In premature infants, the full-term sucking pattern does not appear until the 37th postconception week (104) and it seems to emerge then as a consequence of maturational processes. Gryboski (145) found that immature sucking occurred in conjunction with poor peristalsis among premature infants, while full-term sucking seemed to emerge concurrently with changes in the functioning of the esophageal sphincters. The amplitude, rhythm, and persistence of sucking in full term newborns is noticeably different when the infants' feed consists of dextrose solution rather than milk (101, 102). Of additional importance to research workers is the information that amount of food intake and conditions surrounding labor and delivery (including administration of anaesthesia to the mother) are related to nutritive but not to non-nutritive sucking (103).

Other studies of sucking concerned more general problems in the regulation of infant behavior. Simner (334) explored the hypothesis that stimulus feedback from the fetal heartbeat enhances the discriminative value of postnatal stimuli that oscillate at a similar rate. Newborn babies exposed to a light that flashed 150 times per minute (a rate within the average fetal heartbeat range) showed a greater increase in sucking rate than did infants exposed to either slower or faster light flashes. Siqueland & DeLucia (337) showed that contingent visual stimulation (e.g. patterns and faces) functions as reinforcement for sucking at both 4 and 12 months, a finding similar to Rovee & Rovee's (308) results with conjugate visual reinforcement of neonatal foot thrusting. Watson (372), however, reported that the visual stimuli he used (changes from abstract patterns to schematic faces) were effective reinforcers of fixation only for boys; auditory stimulation was a better reinforcer for girls.

Another group of studies was directed at exploring the limits of conditionability of the infant. Connolly & Stratton (71) showed that stabilizing the experimental setting reduces variability but does not change the general level of conditioning. Naito & Lipsitt (265) showed that eyelid conditioning can be achieved somewhat earlier than previously thought (at about 40 days), but this phenomenon still seems relatively unstable in such young subjects. Further, the Soviet hypothesis that there is a fixed developmental sequence in which stimulus modalities become effective in classical conditioning received little support from a review by Brackbill & Fitzgerald (37). It seems that the relative effectiveness of a given stimulus depends on the response being conditioned.

Work on preverbal vocalizing showed that: (a) noncontingent exposure of the infant to pure vowel sounds or to consonant-vowel combinations reduces congruent vocalizing by the child (375); (b) the output of both vowels and consonants can be increased differentially by means of contingent social reinforcement (306); and (c) mothers exert increasing reinforcement control over different phonemic classes during the child's first year of life (366). A study by Sheppard (327) purported to demonstrate schedule effects in the operant control of one infant's vocal and motor behavior, but the use of innumerable shifts in both procedure and paradigm circumscribe the implications of this particular effort.

Social behavior.—In 1951, Sears (319) proposed that interpersonal relations are best conceptualized in dyadic terms, i.e. with reference to the fact that two or more people are involved. This point of view has been endorsed widely, but has had little impact on socialization research since the prevailing perspective continues to emphasize parental influences upon children rather than the reverse. In opposition to this tradition, Stern et al (348) completed a factor analytic investigation of the interaction between year-old babies and their mothers, finding high reciprocity in these interactions. Both mother and infant behavior ratings loaded on each of the factors emerging from the analysis. In addition, Pedersen & Robson (283) described certain reciprocal qualities in the interactions of fathers and their infants.

Other work (Moss, Robson & Pedersen 261) dealt with dyadic gazing (mutual visual regard by mothers and their babies). Measured at 1 and at 3 months, the amount of this activity was positively correlated with approaches made to strangers at 9 months for boys but not for girls. Sex differences were also found in the relation between prenatal maternal behavior and postnatal interaction between mother and child (Robson, Pedersen & Moss 300); prenatal maternal enthusiasm was associated with high affectionate interaction with male babies, but with high visual and auditory stimulation of female infants. Finally, Goldberg & Lewis (141) reported sex differences in the actual patterns of mother-infant interaction. Year-old girls were touched more by their mothers, vocalized to them, and were more upset and immobilized when separated from them. Clearly, gender differences require close attention in studies of parent-infant interaction.

In spite of increased interest in the reciprocity of the infant's social experience, relatively little is known about the direction of effects in the infant's interactions with others. No one doubts that the infant himself is a powerful instigating and reinforcing stimulus. Spectrographic analysis has shown, for example, that newborn crying contains considerable capacity for differentiated communication (Wolff 390, Tonkova-Yampol'skaya 359), and Rheingold (295) has listed several ways in which the infant probably socializes other people more than he himself is socialized. Nevertheless, the beginnings of the socialization process are poorly understood owing to the paucity of research dealing with the impact of the infant on other people.

Although interest in attachment behavior is at an all-time high, only a few research progress reports appeared (Caldwell & Mozell 51, Ainsworth & Wittig 6). Fraiberg (127), however, discussed two important issues with respect to early social attachments; (a) the conceptual confusion between libidinal object constancy (cathexis) and object permanence; and (b) the seemingly incongruous observations that discriminate social responding, including reaction to the mother's absence, occurs during the first year of life even though evocative memory (as conceived by Piaget) seems not to appear before the middle of the second year. Fraiberg believes that the effects of internal needs on the development of mental representations do not follow a timetable different from the effects of contact with external objects. According to her view, recognition memory rather than evocative memory must be involved in the various forms of discriminate social responding observed during the first year of life.

Rheingold published a series of elegant studies concerning the role of stimulation (including that provided by the mother's presence) in supporting and inhibiting the infant's exploratory activity. A report presented at the fourth and final Tavistock Seminar reveals the effects of placing 10-month-old infants briefly in a quiet, homogeneous environment. This procedure inhibited exploratory behavior and evoked distress; in contrast, the mother's presence supported exploration and vocalizing (294). In another study, infants were observed first in the presence of their mothers, and then toys were placed in the room for some subjects. The results were described succinctly: "The experimental subjects manipulated the toys and did not fuss. The control subjects fussed and manipulated their mothers . . ." (Rheingold & Samuels, 297, p. 520). Thus, diminished stimulation appeared to make the baby "tired of the environment," although the results also suggest that increases in certain forms of stimulation (toys) serve to activate exploratory activity and, simultaneously, the infant's separation of himself from his mother. Results of a final study (296) were consistent with this latter notion. Ten-month-old babies crawled faster from their mothers into another room containing a toy and remained there longer than when no toy was placed there. Further, they stayed in the room with the toy longer during a first trial than during a second, and longer with three toys than with one. The findings of Lewis & Goldberg (215) point to one possible origin of individual differences in responsiveness to unvarying stimulation—mother-child interaction. The extent to which 3-month-old infants gave sustained attention to a single blinking light was negatively correlated with the mother's disposition to stimulate her infant.

Morgan & Ricciuti (259) studied changes between 4 and 13 months of age in the infant's reactions to strangers. Age differences were found, depending on the nature of the testing situation. When tested in the mother's lap, older babies were more "neutral" and less "positive" than younger ones, but when testing was completed at some distance from the mother the change was from "positive" to "negative" reactions. Masks elicited in-

creasingly positive response across this age period. Robson et al (300) reported that boys manifest stranger fear later than girls do (at 9 months as opposed to 7), but no sex differences were found by Bronson (42).

Each of the foregoing studies involves a "social" referent but few of them were derived from "social" theorizing. Increasingly, developmental psychology is characterized by theoretical integration, and nowhere is this more apparent than in work with infants. The research just cited can be regarded as a portion of the general literature on attentional development as well as a part of the literature on socialization. Because this material should be evaluated in a broader context, our comments on perceptual development begin with the topic of visual preferences in infancy.

PERCEPTION AND COGNITION

Visual preferences in infancy.—Much visual preference research has been directed at defining the stimulus properties that attract attention. Recent reports, however, fail to supply consistent support for the popular view that young infants fixate longest on displays of intermediate complexity. Horowitz (162) found little regularity at less than 14 weeks of age in fixation to checkerboards of varying complexity and, moreover, fixation patterns were not stable from week to week. After 14 weeks, fixation time was a linear function of complexity. This last finding was also obtained by Caron & Caron (59), who found in addition that visual fixation of the checkerboards declined on repeated presentation. This habituation was faster when less complex patterns were used.

Karmel (187) obtained a curvilinear relation between fixation time and complexity, but lesser preference for the most complex stimuli occurred only when checkerboards with very small elements were used. In addition, 20-week-old infants preferred to look at stimuli of greater complexity than did 13-week-olds. Karmel argued that preference research using checkerboard displays has confounded complexity (i.e. stimulus uncertainty) with amount of contour, and he suggested that amount of contour better describes the dimension underlying visual preferences than does complexity. Supporting this contention, he found that different random arrangements of the black and white elements of the checkerboard had no effect on preferences. Similarly, Moffett (256) demonstrated that the number of lines in a display, rather than the regularity or randomness of their arrangement, determined length of fixation among 3-month-olds. Karmel (186) has offered a physiological account of these age differences in visual preferences based on the notion that, with maturation, the receptive cell fields responsive to contour information grow smaller. This reductionistic hypothesis has considerable appeal, but it applies solely to preferences among displays involving contour variations. Cohen (69) used a moving light (which, of course, did not vary in contour) and in this instance, too, infants preferred to fixate displays having an intermediate level of stimulus change. Length of fixation decreased over trials, with preferences shifting toward greater

stimulus change. There was also some evidence that habituation was slower to the displays with the greatest amount of change.

Haith, Kessen & Collins (148) varied the predictability of a light's movement, holding rate of change constant. Sucking and body movements, rather than looking, were used as indices of attention. Movement was suppressed by both the simplest and most complex displays, but was increased by an intermediate level of predictability. This finding was unexpected since suppression of ongoing activity is usually regarded as an index of attention. These infants then apparently attended *least* to intermediate level stimuli.

Two studies of responses to face-like stimuli suggest that smiling may be a more sensitive measure of discrimination than looking. The ability to discriminate facial features was implied by Ahrens' (4) earlier finding that 3-month-old infants smiled more at a face with no mouth or nose than at a face with no eyes. Now, using fixation times, Wilcox (384) has found that visual preferences among groups of 1- to 4-month-olds are determined solely by the number of features in the display. Furthermore, realism seems to have little effect on looking since schematic and symmetrically scrambled faces were fixated equally. Confirming these results, Lewis (212) obtained no differences in time spent looking at schematic and scrambled faces by 3-month-olds, 9-month-olds, and 13-month-olds; only 6-month-olds preferred realistic displays. Of special interest, however, is the fact that smiling and vocalizing were consistently greater in response to the realistic displays. On the basis of these results, Lewis suggested that visual fixation may be determined by schema development, while smiling and vocalizing may be based either on an innate releasing mechanism or on social learning.

Heart rate studies of attention.—An increasing variety of autonomic parameters is being used to measure attention in infants. Employing both heart rate and visual fixation, McCall & Melson (239) tested Kagan's (184) hypothesis that, beginning at about 3 months, infants attend maximally to stimuli which are moderately discrepant from the schemata they have acquired. After a standard stimulus pattern was presented repeatedly, three patterns (scaled in similarity to the standard) were presented to the subject. Visual fixation was not affected by degree of discrepancy, but heart rate showed a deceleration to the least discrepant pattern, no change to the intermediate pattern, and acceleration to the most discrepant. Replication of these suggestive findings hopefully will come quickly.

In a careful study of the effects of auditory stimulation, Clifton & Meyers (67) found that 4-month-olds showed cardiac deceleration to the presentation of a new stimulus. This pattern differs from that found for newborns, who usually show heart rate acceleration to new stimuli. Such a shift, it can be hypothesized, may reflect maturation of the orienting response. In this study, the amount of deceleration habituated with repetition of the stimulus, but when a different stimulus was presented, heart rate remained stable and did not return to a decelerating pattern. It is possible

that these changes in stimulation were too large to elicit the infant's attention, although it also may be that heart rate is a much more uncertain measure of infant cognitive functioning than has been assumed.

Lewis & Goldberg (214) and Dodd & Lewis (96) employed several different measures of orienting to study habituation and dishabituation among 3-year-olds. Habituation with repeated presentation of complex stimuli was slower than with simpler configurations (Dodd & Lewis 96, Caron & Caron 59, Cohen 69), which supports Sokolov's view that rate of habituation reflects the speed with which a neuronal or memorial model of the stimulus is acquired. Lewis, Goldberg & Campbell's (216) finding that degree of habituation increases during the first year of life also suggests developmental growth in the ability to form such a model.

Effects of novelty.—Several studies with older children show the effects of stimulus novelty on orienting responses and choice behavior. Cantor & Kubose (56) found that preschool children looked at "liked" unfamiliar displays more than familiar ones. Endsley & Kessel (112) obtained similar results, but surprisingly, 30 seconds of prior exposure reduced these preferences by an amount as great as 5 minutes of familiarization. Cantor (54, 55) summarized the research demonstrating that familiarity slows reaction time, concluding that increased experience with a stimulus beyond a short exposure has little further effect on response speeds. Novel stimuli, he suggested, evoke an orienting reaction that speeds responding but which habituates with exposure. Supporting these conclusions, Miller (249) reported that familiarization decreased the latency of the initial part of the response only. However, not all novelty effects are attributable to orienting responses. Cantor points out that in disjunctive reaction time tasks, the tendency to respond faster to a changed stimulus, as opposed to a repeated one, may be due to the subject's expectation of the change.

Although children may look more quickly and look longer at unfamiliar objects, it would be biologically reasonable to hesitate before approaching such objects. In point of fact, preschoolers have been found to approach an unfamiliar climbing apparatus with a longer latency on a first exposure than on a second (Jeanrenaud & Linford 177). Such temporary avoidance of unfamiliar objects evidently appears early in life. Schaffer & Parry (317) found that repeated presentation of an object produced both decreases in looking at the object and increases in looking at a new object among both 6- and 12-month-old babies. The 12-month-olds hesitated for about 10 seconds in reaching for the object during early trials, but with familiarization, the latency of reaching decreased. The younger infants, on the other hand, did not demonstrate this integration of looking and reaching behavior. They reached for the object immediately, regardless of its familiarity.

Visual preferences in older children.—Hutt & McGrew (170) found that 5-year-olds looked longer at polygons with relatively few sides while 11-

year-olds preferred many-sided figures. In contrast, Faw, Nunally & Ator (119) detected no changes from middle childhood to adulthood in looking time as a function of stimulus uncertainty. Dot patterns with the greatest uncertainty were fixated longer only when the subjects were specifically instructed to identify the pattern. This suggests that aesthetic preferences are less important in determining fixation than time required to process the pattern.

Berlyne (27) has hypothesized that highly incongruous displays are not preferred for viewing because they arouse the individual above an optimal level. Nevertheless, Nunally, Faw & Bashford (273) obtained a monotonically increasing function between viewing time and pictorial incongruity among both 3rd-graders and adults. It is possible, however, that pictorial stimuli can never be sufficiently incongruous to test this hypothesis satisfactorily, and three-dimensional objects should perhaps be tried.

Thomas (355) found that children and adults both reported "liking" dot patterns containing many rather than fewer dots. When numerosity was held constant, however, children chose redundant patterns and adolescents preferred those with intermediate variability.

Dimensional preferences.—Awareness that certain properties are particularly useful in identifying objects develops gradually and may underlie the age changes in dimensional preferences obtained in recent research. Gliner et al (140) reported that kindergarteners preferred texture when learning a tactual discrimination, while 3rd-graders preferred shape. When the same task was presented visually, both age groups attended to shape. These preferences were little affected by decreasing the discriminability of the shapes, although the younger children shifted from texture to shape in the haptic conditions that involved less discriminable textures. The results of this study, based on stimuli that were psychophysically scaled, thus rule out the hypothesis that dimensional preference is simply a function of discriminability.

Earlier work with certain African populations has failed to confirm the often observed increase with age in preference for form over color. Serpell (322), working in Zambia, replicated this finding with rural but not with urban children. The difference between these two groups does not seem attributable to linguistic differences in the codability of color and form. On the contrary, the results of a second study (323) suggest that it is Western education that instills a preference for geometric form, perhaps through the use of "form-board inspired" toys.

Caron (58) and others have suggested that developmental changes in both discrimination learning and in shift behavior are influenced by attentional preferences. Consequently, considerable effort is being expended on descriptive studies of these preferences. Ginsberg (139) found that 5-year-olds attended to number rather than color in a successive discrimination learning task, and it seems clear that number rather than form or density

was the preferred attribute. Other studies showed that: (a) preference for size develops earlier than preference for number (39); (b) 2nd-graders prefer form to both color and number (254); and (c) 3-year-olds prefer brightness to height (58). Landau (204) suggested that the tendency for younger children to produce flatter generalization gradients than older children may be due to differential dimensional preferences rather than to lack of specificity in discrimination. Unlike 4-year-olds, the 3-year-olds in that study generalized maximally to all stimuli on a size continuum even though they spontaneously labeled the stimuli as "big" or "little," thus indicating an appreciation of size.

Selective attention.—Until recently, anecdotal evidence provided the major support for the hypothesis that selective attention improves with age. Maccoby's research (232) has done much to remedy this situation, and her review of recent work contains evidence of developmental improvement in both external attention (i.e. the efficiency of sense organ adjustments) and internal attention. Attempting to specify the processes underlying these age changes, she concluded that improvement in selective listening is not due to a response artifact, to peripheral masking, nor to the inability of young children to adopt a selective set in advance of stimulation. Instead, she suggested, it is the basic ability to discriminate a stimulus in a noisy background that improves with age.

Maccoby also hypothesized that breadth of attention—the ability to attend to more than one thing at a time—increases developmentally. Supporting this contention, Eimas (107) found that, when presented with a discrimination learning problem including several relevant and redundant cues, kindergarten children learned one cue, 2nd-graders two, and 4th-graders responded to three. Some subjects as young as 2 years of age, however, can apparently be trained to attend simultaneously to two attributes of objects. Watson & Danielson (373) used a shaping procedure and successfully trained 4 out of 24 2-year-olds to sort objects on two dimensions.

Developmental changes in breadth of attention are also implicated in the finding that the relation between age and incidental learning is curvilinear, with the peak occurring about the 10th year. Siegel & Corsini (329) found the amount of such learning to be equal on either side of the peak (at 8 and at 14), but different processes appear to be involved at these ages. When a conceptual relation was established between the relevant and irrelevant stimuli, incidental recall increased but only among the older subjects. Thus, restricted attention could account for the performance of younger subjects but not for the performance of older subjects, who processed the irrelevant information minimally. Similarly, decreases in incidental learning after age 10 does not reflect improvement in visual selectivity. Druker & Hagen (100) found that widening the spacing between the central and peripheral stimuli diminished incidental recall equally among 10- and 13-year-olds, but post-test questionnaires revealed that only the

younger subjects rehearsed the relevant and irrelevant stimuli indiscriminantly. In sum, developmental change in breadth of attention may underlie the initial increment in incidental learning, but the subsequent decrease seems to be a matter of differential processing and rehearsal of relevant and irrelevant material rather than differential attention.

Improvement with age in the efficiency and speed of visual scanning is also an important aspect of attentional development. 8-year-olds were shown to be substantially less able to detect a figure hidden in a strong gestalt than were 14-year-olds (291), although search time for both age groups was slowed by an organized background. In a similar study, Spitz (344) found that reducing the information distinguishing a target stimulus from a group of background items slowed scanning rates more for young subjects and retardates than for older subjects.

Differences in the speed with which visual fixations can be shifted from component to component probably contribute to these developmental changes in scanning. Miller (251) found that the latency of saccadic eye movements to the presentation of a light was nearly twice as long for 8-year-olds as for college students. Although developmental studies of visual scanning have just begun, they should be extremely useful within the larger context of research on information processing, especially if the properties of the target and background are thoughtfully manipulated.

Perceptual processes.—Pollack (286) has concluded that two processes underlie age changes in perception. One is the progressive loss of receptor sensitivity due to physiological aging. This process is thought to account for the decreasing effectiveness of illusions based on contrast, intensity, duration, and orientation. The second process is correlated with IQ and involves the temporal integration of separated events. To test this position, the threshold for detecting an interval of darkness between two light flashes was investigated with children aged 6 to 17 years (287). It was expected that the decline in threshold due to receptor aging would be reversed among the older subjects because they would be better able to integrate temporal events. Instead, the threshold declined linearly with age. In another study, no change in visual acuity was found for subjects 7 to 14 years old (339). To date, the source of the discrepancy between these and earlier findings has not been established.

Two studies demonstrated that slight procedural variations have a large effect on performance in perceptual tasks. Using a discrimination problem in order to study size constancy, Rapoport (292) found no difference in the extent to which distance affected size discrimination among children between the ages of 5 and 10; unlike adults, all the children showed some underconstancy. In a visual cliff situation, Walk (368) lured the infant down a narrowing runway, forcing him to one side or the other instead of coaxing him across the deep side. This simple change revealed greater sensitivity to depth than that shown by previous work. When the drop measured only 5

inches, 73% of the infants preferred to crawl on the shallow side. On the other hand, most of these infants crawled to their mothers over the deep side when the standard method was used.

Kindergarteners experience some difficulty in discriminating among letters differing in orientation (e.g. b, d, p, and q). Caldwell & Hall (52) believe that this is due to an inappropriate concept of "same" and "different" rather than to any difference in perceptual capacities. Supporting this contention, a training procedure emphasizing orientation produced a marked increase in the percentage of subjects discriminating correctly.

Theoretical perspectives in cognitive development.—Considerable effort is being given to the empirical refinement of Piaget's experimental work, but relatively little thought has been expended on his theory. Furth's *Piaget and Knowledge* (131), however, reflects an appreciation of the complexities of such topics as operative, symbolic, and figurative knowledge; learning and equilibration as mechanisms of cognitive change; and the biological characteristics of the theory. This book also includes several previously untranslated selections from Piaget's own work, supplementing *Mechanisms of Perception* (284) which is now available in English.

Elkind and Flavell (111) edited an important group of essays which underscore the wide range of problems in cognitive development elucidated by Piaget. Hunt dealt with the implications of Piaget's theory for S-R (stimulus-response), psychoanalytic, and other traditional approaches to development. Pinard & Laurendeau analyzed the criteria proposed by Piaget for use of the stage concept, and two other essays touched upon the problem of stage transitions: (a) Flavell & Wohlwill explored the ramifications of Chomsky's competence/performance distinction for research and conceptual refinement in the study of cognitive development, and (b) Wallach examined the nature of the information required to precipitate the developmental shift from nonconservation to conservation. Wallach argued that the sources usually suggested—perceptual input, reversability, and decentration—are either not informative or presuppose conservation itself.² She proposed instead that the equality of two quantities is established if an identical external action makes them perceptibly equal. Elkind attempted to distinguish the nature of concepts, as used in concept identification studies and in Piagetian conservation experiments, while the value of surprise as a measure of cognitive functioning was assessed by Charlesworth. The relation between surprise and attention was explored in detail, with special emphasis being given to the causal role of this reaction in cognitive development.

In addition to the theoretical work appearing in the Elkind & Flavell volume, Jeffrey (178) argued that traditional learning theory has certain

² Murray & Johnson (262) also argue that reversibility of an operation does not imply that a particular property is invariant under that transformation.

deficiencies in accounting for early perceptual and cognitive development. He suggested that much early learning results from the child's unreinforced attention to certain aspects of the environment and that control of attention is a central problem for developmental psychology. Jeffrey's model is based on the serial habituation of cue salience to account for changes in infant scanning and other attentional behaviors. In one other theoretical essay, Pascual-Leone & Smith (281) proposed that quantitative growth of a central "computing capacity" might better account for developmental changes in both problem solving and symbolic behavior than notions about new cognitive "structures."

The sensory-motor period.—Considering the popularity of research on infancy, surprisingly little effort is being given to extending Piaget's investigations of the sensory-motor period. Two major programs of research are addressed to this period of development, however, and both deal with the ontogeny of visually guided reaching. Bruner (47) suggested that when such reaching is first differentiated from gross activation, it is part of a fixed action sequence that always ends with the grasped object being mouthed. Only later is reaching freed from this sequence to become an independent subroutine. Research on looking, sucking, and grasping during the first 6 months of life (White 379) indicates that these schemas change at about the 3rd month from being independent to being coordinated. It was also reported that the rate of sensory-motor development was increased by enriching the environment in which the infants were reared.

Corman & Escalona (72) created three scales of infant cognitive development based on Piaget's stages of sensory-motor intelligence. In a longitudinal study, they found an invariant sequence of behaviors within their scales of prehension, permanence, and spatial relations. There was, however, great variability across scales (i.e. horizontal decalage). This suggests that environmental richness has little bearing on correlated progressions, although such input may be related to the range of situations in which stage-appropriate behavior appears.

Preoperational period.—The preoperational period is largely defined in negative terms, i.e. by the absence of the conservations. Nevertheless, one type of conservation develops in very early childhood—the belief that an object retains its identity in spite of changes in its appearance. DeVries (93) documented this point by disguising a live cat, while the subject watched, so that the cat looked like a fierce dog; 3-year-olds, but not 5-year-olds, believed the cat had changed its identity, as evidenced by fearfulness and verbal response.

Mehler & Bever's (245) report that children demonstrate a type of number conservation as early as 2 years of age continues to elicit rejoinder. By asking children to choose the row of candies that "had more to eat" they

found an appreciation of number independent of length in 2-year-olds, whereas older children tended to base their choices on length. Rothenberg & Courtney (305) also found that preference for length increased with age, but 2-year-olds tended to choose the closest and not the most numerous row. In the standard verbal conservation test only 2 percent of the 2-year-olds conserved number (304). This is not surprising, since Siegel & Goldstein (330) found that children younger than 4 1/2 do not respond correctly to questions using relational terms such as "more," "less," and "same." Thus, conservation of number in the usual sense has not been demonstrated in preoperational children, although an early perceptual appreciation of some aspects of number remains a possibility.

Training and conservation.—A large number of studies, conducted somewhat outside Piaget's framework, were successful in inducing both generalized and durable conservation. It is important to note that generally subjects were employed in these studies who were close to the age at which conservation would normally occur, and Beilin (24) has suggested that the difficulty in inducing conservation in younger children supports the hypothesis that training succeeds only when the child possesses the requisite cognitive "apparatus."

Viewing the acquisition of conservation as a discrimination learning problem, Gelman (134) argued that children who fail to conserve are simply not attending to the relevant features of the task. To test this hypothesis, she trained 5-year-old nonconservers to attend to length and to number as independent dimensions. Post-test data, obtained both immediately and after several weeks, showed a high degree of conservation of both length and number and considerable nonspecific transfer to the conservation of mass and volume. Apparently the attentional training was successful in inducing a general rule that the surface appearance of a display should be ignored. Similarly, Bearison (22) was successful in training conservation of continuous quantity. Nonconservers were given repeated experiences in counting discrete units of volume, a kind of training that may direct attention away from misleading perceptual cues. The effects of this training were apparent immediately and were maintained over a 7-month period; there was also a fair amount of generalization to other types of conservation. Most interestingly, explanations given by the trained subjects were much like those given by "natural" conservers.

Minichiello & Goodnow (252) hypothesized that in typical tests of conservation of volume, the child's attention is drawn to misleading attributes of height at the point in the testing procedure when initial equality of the two quantities is established. To demonstrate this, they asked the child to drop equal units of material into each of two containers simultaneously to create the initial equality. Directing attention to equivalences in this manner produced a significant increase in conservation.

Most authors of developmental studies of conservation and of training

studies assume that children of different ages do not differ in their understanding of the instructions. However, several studies showed that the verbal understanding of preconserving children differs from that of conservers: (a) nonconservers did not differentiate terms referring to height or width (336); (b) they failed to distinguish connotative from denotative meaning in the understanding of weight—i.e. heavy objects were judged to be stronger and harder (272); (c) young children tended to interpret questions about bigness primarily by reference to the height of objects (166); and (d) a training procedure designed to elucidate the word "bigger" improved performance in a weight conservation task but only among children over 6 1/2 years of age (229). All of these findings suggest that the understanding of verbal terms used in conservation testing is a prerequisite for demonstrating conservation. Understanding the terms themselves may depend on the prior development of cognitive structure (Sinclair-de-Zwart 336) but, in any event, this research points up a major problem in evaluating the typical conservation training study.

Another approach to the problem of conservation acquisition is represented by cross-cultural work. Price-Williams, Gordon & Ramirez (290) found that conservation of substance occurred earlier among Mexican children of pottery-making families than among a matched control group from non-pottery-making families. The authors suggested that potters' children had greater experience in manipulating clay, and that this experience was directly related to performance on conservation tests. Interestingly, the differences between these two groups of subjects were specific to conservation of substance—the two groups did not differ in conservation of number, liquid, weight, or volume. Such findings bolster Goodnow's (143) hypothesis that action models, and manipulation, are important components of a conservation-supporting milieu.

De Lemos (92) found that the appearance of conservation is retarded by several years in aboriginal Australian children. In contrast, Heron, & Simonsson (158) obtained an equal increase, from age 7 to 8, in conservation of weight among rural Zambian and European children. However, in both studies the percentage of adults that conserved never exceeded 50. The authors suggested that, in addition to poor schooling, inadequate nutrition might account for the failure of such a large segment of the population to conserve, but in these studies the equivalence of testing methods across cultures was not established.

Concept utilization.—Four studies lend credence to Bruner's hypothesis that children first use perceptual properties and then shift to abstract-functional properties in establishing that objects are similar to one another. First, Crager & Spriggs (77) found an increase with age in the choice of functional properties in matching pictures (e.g. "They all make music"), but no decrease in the use of the perceptual properties such as shape and color. Second, Butters (49) demonstrated that training emphasizing equivalence judgments produced an increase in functional responses, while similar

training that involved viewing and manipulating objects apparently emphasized perceptual properties and produced superordinate responding (e.g. "They all have holes in them"). Both kinds of training decreased "immature" responses. Next, suggesting the importance of the training provided by formal schooling, Maccoby & Modiano (233) compared the performance of urban and rural Mexican adolescents on an equivalence task. Abstract formal attributes predominated in the responses of urban children while concrete perceptible ones were most frequent in the responses of the unschooled peasant subjects. In a similar study conducted in Uganda, Evans & Segall (114) concluded that educational experience rather than age was the critical factor underlying increasing use of functional properties.

If different types of concepts are predominant at different points in development, one would expect to find variations with age in the conditions which maximize productivity in generating concepts. Goodnow (142) showed that kindergarten children produced more nonstandard uses for common objects when they had previously manipulated them, as opposed to viewing them. Whether the effect was due to additional information supplied by the tactual experience, to changing views of the object produced by manipulation, or to activity itself is not known. Younger children also seem to produce generally fewer concepts than older ones when asked to state ways in which groups of objects are alike (110). This is particularly true when object names rather than the objects themselves are the stimuli. Also, the child's skill at finding conceptual equivalences between words seems to be improved by training in which related words are presented in clusters rather than randomly mixed. Such training apparently focuses attention on conceptual equivalence relations (66).

In these studies, concept utilization was examined in situations that did not restrict the type of concept used. In those that follow, however, the child's ability to identify a pre-established concept was assessed. Snow & Rabinovitch (341) examined the hypothesis that the adult's greater difficulty in learning disjunctive as opposed to conjunctive concepts is due to the complexity of disjunctive concepts rather than to differential practice with them. The finding that this difference in difficulty is constant over a wide age range supports the hypothesis.

Saravo & Gollin (313) suggested that the inability of most 4- and 5-year-olds to learn oddity problems is due to interference created by presenting the same set of stimuli for successive problems. When given a learning set procedure in which interference was minimized, only 3-year-olds failed to maintain stable oddity performance. Other evidence that 4- and 5-year-olds can and will use oddity as a cue in discrimination learning was provided by Keeney, Jenkins & Jenkins (191). Trained first on a discrimination in which shape and oddity were redundant cues and then transferred to a standard oddity task, 3-year-olds showed no transfer while the older subjects responded significantly to oddity. Furthermore, training on the redundant oddity task produced more transfer in the older children than an equal amount of training on the standard oddity task itself.

INTELLIGENCE

One of the oldest issues in developmental psychology—nature versus nurture—has precipitated a heated controversy. Arthur Jensen published a broad ranging paper dealing with heredity, IQ, the nature of learning abilities, early intervention programs, and scholastic achievement (179). His review furnished a sound basis for concluding that there is a sizable heritability factor in measured intelligence, but in addition he concluded the following: (a) mean differences in IQ scores between Negroes and Caucasians are primarily genetic in origin; (b) environmental influences could not possibly be sufficient to account for these differences nor early education successful in reducing them; (c) early intervention programs have had limited success and are not likely to succeed because uniform educational models do not match the genotype in all segments of the population; (d) children's learning abilities are of two distinct types (rote learning and conceptual learning) which appear in a hierarchical progression that is subject to genetic constraint.

Rebuttals were directed at all of these arguments (80, 81, 109, 167, 169, 183). Least vigorously questioned were Jensen's conclusions about the inheritance of intelligence, but his inferences concerning the genetic origins of racial differences in IQ, the efficacy of early childhood education, and his analysis of children's learning abilities all came under heavy attack.

What has been the impact of this debate? In our view, it has added very little to developmental science. No new conceptualizations, research questions, or conclusions have come from it. The impact of the controversy on public attitudes and policy making, however, is more difficult to assess. There was no dearth of responsible argumentation in this exchange, but the best of it appeared in professional journals while the worst appeared in the mass media. Hasty and unwise decisions could be made on the basis of these arguments, and we doubt that the population at large is any better informed about the complexities of nature and nurture than previously.

Among recent empirical studies of intelligence was a series of inter-correlational investigations. The practice of "streaming" in British schools provides masses of psychometric data on school-age children, and each year adds further information about the factor structure of intellectual abilities (e.g. Butcher 48). Stimulated by current interest in cognitive development, Dudek et al (105) completed a 3-year longitudinal study of the relation between WISC IQ and a battery of Piagetian tasks designed to measure various aspects of preoperational and concrete operational thinking. Correlations ranged between .52 and .62 for children between the ages of 5 and 8. Saltz & Hamilton (312) found that IQ was more strongly related to the probability of ultimately solving a concept formation task than to the rate with which problems were solved, further indicating the complexity of the relation between measured intelligence and concept learning. A number of validation studies also appeared on the recently developed *Wechsler Preschool and Primary Scale* (21, 116, 242). Correlations between scores on this test

and the Stanford-Binet ranged around .80 while correlations with the *California Test of Mental Maturity* ranged around .50.

Werner (378) explored the hypothesis that intellectual performance is more sensitive to environmental influence among boys than among girls. Data from an Hawaiian sample tested at 20 months and 10 years showed that the IQs of girls only were significantly correlated with parental IQs at 20 months, but the IQ scores of both sexes were correlated with parental stimulation at both age levels. Thus, the data provided no support for the hypothesis. A sex difference of another kind, however, was reported by Rosenberg & Sutton-Smith (301). Being first born and having widely spaced siblings was associated with high scholastic aptitude among males, while having like-sexed siblings and close spacing was associated with better performance among females. Overall, however, family density seems to be negatively correlated with children's IQs (88).

The combined influence of both genetic and milieu factors is graphically depicted by Polansky et al (285), who studied a group of poor families in southern Appalachia. They found a positive correlation between maternal and child IQs but also between the mother's IQ and the adequacy of her child-caring. The confounded influence of both genotype and environment in determining intellectual capacity was also revealed by a study conducted in rural Mexico (79). There tall and heavy women, producing tall and heavy babies, were over-represented among families having higher incomes, smaller families, and better houses; smaller women, producing smaller babies, were over-represented among less favored societal groupings.

Data from most of the major early childhood intervention programs are still buried in progress reports. Project Head Start is now 5 years old, and this period is not long enough to generate the data needed to evaluate the effects of such programs on behavioral development (White 380). Some reports appeared showing that very different curricular approaches lead to fewer differences in cognitive and verbal functioning than might be supposed (320). The only relatively complete evaluation report to appear, however, concerned the sequential learning program developed by Van De Riet and his associates (364). This program produced a wide variety of gains, measured both at the end of kindergarten and at the end of first grade among children attending the experimental school. These gains were greater than those shown by a control group attending a "traditional" kindergarten and a control portion of the original subject pool who remained at home. The next several years will undoubtedly see many more formal reports on such experimental programs. In the meantime, an important two-volume work, including a teachers' manual, described preschool education in the Soviet Union (62, 63).

LANGUAGE

The acquisition of syntax.—There were disappointingly few studies of morphological-syntactic development this year, but the longitudinal

research of Brown and his associates (46) is extraordinarily rich in both data and hypotheses. The range of topics covered by that report is broad, including the innateness hypothesis, the influence of parental speech, and the grammar-writing approach to early syntax. The data are focused on the period of telegraphic speech. Although segmenting speech into words is a significant first step in syntactic development, it is rarely discussed, perhaps because children make so few segmentation errors. Brown et al presented evidence that such errors occur at boundaries unmarked by pauses and only when the conjoined words have high transition probabilities in parental speech. Basic grammatical relations (e.g. subject of, modified by) are expressed correctly from the beginning, but the transformational system is rudimentary; even at the end of this period, questions are apparently produced by applying only one of the requisite transformations. Grouping words into grammatical categories (e.g. nouns, verbs) proceeds with remarkable accuracy, even when no semantic differences underlie the distinctions. Francis (128) published a thorough description of the syntax of a 2-year-old, emphasizing the distribution of grammatical structures in various settings. Also, Miller & McNeill's (250) review of the psycholinguistic literature includes a section on acquisition.

Two papers offered valuable heuristic suggestions. Sinclair-de-Zwart (336) outlined the congruencies and incompatibilities between Chomskian and Piagetian views on language development. Her suggestion that language acquisition be considered within the framework of the child's total cognitive development may serve to correct the deadening influence of an over-extended innateness hypothesis. Carlson & Anisfeld (57) argued that the study of syntactic development would be enriched if a wider range of linguistic phenomena were considered; they provided examples of accurate generalization of word meaning and spontaneous substitution of words in nursery rhymes to support their contention.

Studies of syntactic facilitation of paired-associates learning confirm the importance of deep structure relationships. Second-graders were better able to learn noun pairs representing deep structure subject and object (in passive sentences) than those having no integral relationship (in sentences with an adverbial by-phrase) (Davidson & Dollinger 89). Also, noun pairs are easier to learn when linked by a verb than when linked by a conjunction, and this is apparently not a function of the subject's familiarity with various sentence types (351).

Gallagher (132) also used a paired-associates task to study semantic development. Meaningful pairs were learned with fewer errors than non-meaningful ones by all but the 5-year-olds; this difference increased to age 8. Superior learning of noun-verb and adjective-noun sequences over verb-noun sequences occurred only with meaningful pairs, suggesting that semantic factors may influence performance more than syntactic ones in tasks of this kind. A note on the syntagmatic-paradigmatic shift in word associations: Masters (237) found a small correlation between syntagmatic association and the tendency to give functional word definitions.

Phonological development.—Most of the year's research on phonological development dealt with the nature and source of articulation errors (e.g. Locke 221). A more central question concerns possible mechanisms underlying the acquisition of speech sounds; one hypothesis is that the articulatory movements, assumed to be important in perceiving speech, also play a role in acquiring the relevant perceptual distinctions. Menyuk (248) found that 4-year-olds observed phoneme boundaries more often when identifying than when repeating synthesized syllables. In contrast, among older children and adults, identification and reproduction responses tended to converge. Menyuk's tentative conclusion is that articulatory differences probably do not determine the child's initial perception of speech sound categories.

Communication.—Ervin-Tripp's (113) review of sociolinguistics reveals the complexities of verbal and nonverbal communication and, in so doing, indirectly suggests numerous questions worthy of developmental study. Attempting to pinpoint the source of age differences in communication accuracy, Krauss & Glucksberg (203) evaluated messages in terms of such indices as type-token ratio and commonness of vocabulary. No difference among messages from different-age subjects was obtained, although adult listeners found messages from older subjects much more adequate.

The remaining communication studies focused primarily on the receptive abilities of children from different social classes. Two compared responsiveness of middle- and lower-class children to instructions and verbal reinforcement delivered with varying tonal inflection. Middle-class children responded similarly to instructions given in positive, neutral, or negative tones of voice, while lower-class subjects responded best to a positive tone (Kashinsky & Wiener 188). In addition, tone did not affect the reinforcing value of words for middle-class children unless it was incongruent with the message; on the other hand, tone was very salient to lower-class children, for whom words spoken in a neutral tone of voice had no value as reinforcers (Brooks et al 44).

Baratz (20) found that lower-class Negro and white children behaved similarly when asked to imitate sentences in standard English and in Negro dialect: accuracy was poorer on sentences outside the subject's own dialect and errors consisted overwhelmingly of recodings to the subject's own dialectal forms. Baratz reviewed the major positions on language deficiency in disadvantaged black children in light of these data, concluding that these children possess a fully structured language but are not bidialectal, as their educational situation generally requires. However, that subjects recode to their own dialect seems to imply at least receptive bidialectism, an interpretation confirmed by Weener's (376) finding of no significant difference in repeating standard and nonstandard English by a nonstandard dialect group. The source of the discrepancy between these results, however, is unknown. Tests of comprehension and detailed error analyses with respect to a variety of linguistic features are obviously needed.

The spontaneous speech of middle-class Negro and white children appeared more mature than that of lower-class children on frequency-based indices of syntactic development, but less so on hierarchical indices (Williams & Naremore 386). Nearly all significant social class effects were accompanied by race or topic interactions, implicating the communication situation as an important factor in comparing social classes.

LEARNING AND MOTIVATION

Discrimination learning.—The debate between attention-oriented and mediation-oriented theorists dominates developmental work on discrimination learning. A number of studies dealt with the role of dimensional preferences, and there seems to be good agreement that these preferences predict ease of discrimination learning in children ranging from 3 to 8 years of age (58, 87, 176, 360). On the other hand, developmental differences in extradimensional shift (EDS) and intradimensional shift (IDS) performance may be due to a change in the subject's probability of attending to the relevant dimension rather than to his use of linguistic mediation. If so, attentional preferences should have a large effect on shift performance.

Caron (58) found that 3-year-olds, for whom verbal mediation seems unlikely, learned ID shifts more rapidly than ED shifts when original training was on the subject's preferred dimension. When sensitized to their non-preferred dimension and then trained on it, they also learned the ID shifts more rapidly. Campione (53) obtained essentially the same results with retarded children, MA=50 months. Both investigators found that the two shifts were learned with equal ease when training involved the nonpreferred dimension. Trabasso et al (360), who studied upper-middle-class 4-year-olds, and Dahlem & McLaughlin (87), using 1st-graders, found that subjects trained on the preferred dimension chose to make an ID shift more often than those trained on their nonpreferred dimension. Wolff (389) obtained similar results with 4-year-olds, assessing preferences through the subject's use of dimensional labels.

Unfortunately, evidence concerning the relation between preferences and shift performance is not as consistent as the above studies would suggest. When Trabasso et al (360) repeated their study with lower-middle-class subjects, preference had little effect. Children who preferred color learned the original discrimination more slowly and made ED choices regardless of the dimension on which they were trained. Form-preferring subjects tended to make ID shifts regardless of which dimension was initially relevant, and James et al (176) found that training on a preferred dimension had little effect on the shift choices of kindergarten children. These findings imply that developmental changes in shift performance may reflect more than just increased differentiation of dimensions that the subject attends to.

Attempting to answer recent criticism of their mediational analysis of age changes in reversal shift behavior, Kendler & Kendler (192) argued that the effectiveness of such variables as dimensional preference within an age

group do not imply the absence of developmental differences. In fact, when dimensional preferences are controlled, the choice of a reversal shift in optional shift tasks continues to increase with age. Using conceptual categories rather than stimuli that share various perceptual attributes (thus precluding an attentional account), Kendler, Kendler & Marken (193) trained subjects (4 to 18 years) to sort related pictures—e.g. animals vs fruits. To index use of this conceptual organization, a reversal shift was compared to a shift that destroyed the possibility of responding on the basis of superordinate categories. Four-year-olds apparently failed to use the common conceptual property, while older subjects increasingly did so. This leaves linguistic mediation as the only plausible account. However, the possibility remains that 4-year-olds would mediate if instructed to, and if categories familiar to young children were employed (Steinmetz & Battig 347).

The preference measure itself both may influence and be influenced by concept identification training, especially in younger children. Kindergarteners, but not 8-year-olds, found learning on their nonpreferred dimension more difficult when preceded rather than followed by the preference measure (Mitler & Harris 254). In addition, administration of the preference measure following training substantially changed the preference patterns of the subjects. Apparently each task creates an attentional set that carries over to the other.

Following Gibson's differentiation theory of perceptual learning, Tighe & Tighe (356) pretrained subjects to attend to the dimensional rather than the object character of the stimulus. During pretraining their subjects made "same-different" judgments, without feedback, about objects varying along the dimensions appearing in the transfer task. Surprisingly, pretraining had little effect on discrimination learning but did facilitate IDS performance. When the number of irrelevant dimensions was increased, only the performance of the non-pretrained subjects suffered.

Several studies dealt with the role of linguistic mediation in transposition and shift behavior. The Tighe's investigated the effects of perceptual pretraining and labeling on transposition behavior and reversal learning. They found that verbal labeling, when added to perceptual pretraining, did not improve performance on a discrimination reversal or on a transposition test (357). In another study, dimensional pretraining with at least three values appeared to be necessary to facilitate transposition (358). Increases in transposition with age, they contended, reflect increases in the child's tendency to view training and test pairs as belonging to the same stimulus dimension.

Cole et al (70) found additional evidence against the linguistic mediation account of transposition. African children, whose language contains an asymmetry in the expression of size comparisons not found in English, were compared to American children in the transfer of a learned discrimination along the size dimension. Transfer choices were not related to differing

linguistic patterns, although post-test descriptions of these choices were. In addition, the relation between linguistic ability and optional shift choices was explored by Andre (11), using 7- and 12-year-old hearing and deaf children. No effects of hearing status were found, but the older subjects learned the discrimination faster and were more likely to choose a reversal shift.

Attentional theory predicts that increasing the number of irrelevant dimensions varying within trials lowers attention to the relevant dimension and impedes concept identification. This prediction was confirmed over a wide age range for oddity problems (228), simultaneous discrimination problems (95, 227, 356), and transposition (85). Also, Brown (45) found that introducing novel stimuli into a learned two-choice discrimination problem disrupted children's performance. The hypothesis that there are age changes in the distracting effects of novelty was supported by the larger effect obtained for 4- than for 7-year-olds. A similar result was obtained by Rosenberg & Birch (302). Also, the modality in which reversal and nonreversal shifts are presented influences attention to the dimensional nature of the stimuli. Bloom & Moore (33) reported that a tactual presentation enhanced the shift solution in a reversal situation, while interfering with the solution in a nonreversal task.

In summary, neither an attentional nor a verbal mediational account successfully deals with developmental changes in discrimination learning. Perhaps consideration of the problem within the broader context of cognitive functioning will provide some insight into the processes underlying age differences.

Concept identification.—There is increasing evidence that children learn simple discrimination tasks in an all-or-none fashion. Not even children with low IQs improve gradually on such tasks, as demonstrated by Hamilton & Saltz's (150) study of 8-year-olds who ranged in IQ from 75 to 140. Similarly, Levin & Maurer (211) found that mastery of match-to-sample problems involved an abrupt shift from chance responding to perfect performance, and Rieber (298) found that kindergarteners who failed to learn a concept identification task showed no increase in correct responses over trials. These findings are consistent with the notion that a process of hypothesis generation and testing occurs, even in very young children.

To explore this view further, several investigators used Levine's partial feedback procedure to make inferences about the hypotheses being employed by the subject. Using children from 5th grade through college, Ingalls & Dickerson (172) found little random responding, and there was a reduction, over trials, for all age groups in the number of hypotheses sampled. Younger subjects proved less efficient in problem solving, and they reduced the set of hypotheses (focusing) more slowly than did older subjects. Eimas (106) also found that 2nd-grade children formulated and used hypotheses, but again, focusing was much less evident than among older children. The

younger children were also more likely to persevere on an incorrect hypothesis than were the older children. Finally, younger subjects seem to require practice with the task in order to discover the utility of hypothesis testing; such behavior seems to be absent during the early trials of discrimination experiments (e.g. Rieber 298). Thus, it is clear that children rarely respond randomly in problem-solving situations; even 5-year-olds produce patterned sequences of responses. Consideration of Piaget's work on the acquisition of a hypothetical attitude, however, plus the failure cited here to find much focusing among young children, ought to temper our willingness to attribute sophisticated hypothesis-testing abilities to young children.

Memory and verbal learning.—Theorizing about verbal learning and human memory is being aided by the results of research with children. Developmental studies have helped clarify the nature of information processing by capitalizing on the fact that in learning young children do not utilize many of the active strategies employed by older children and adults. For example, Belmont & Butterfield (25) hypothesized that the superior short-term memory of older or more intelligent subjects is due to their use of more active acquisition and retrieval strategies. Using hesitations in self-paced learning to index covert rehearsal and chunking strategies, they found that 6th-graders and retardates were less likely than college students to organize actively a sequence of items. Similarly, Daehler et al (84) obtained an increase from kindergarten to 4th grade in spontaneous verbal rehearsal when the order of a set of events was to be learned. On the other hand, rehearsal of spatial positions by pointing failed to mediate recall and showed inconsistent age trends.

The tendency to rearrange pictures of objects spontaneously during learning so that members of the same conceptual category are grouped together is greater among 5th-graders than among younger subjects (Moely et al 255). However, instructions to reorganize the pictures produced a decided increase in recall for younger children, supporting a production deficiency rather than a mediation deficiency hypothesis. Other support for the production deficiency hypothesis was provided by Silverman & Craig (332).

Several studies emphasized that under some conditions young children seem as effective in their use of structures to aid learning and recall as older children do. First, Horowitz, Lampel & Takanishi (163) found that memory for a missing object in a display is nearly perfect in preschool children, if the objects are originally related to one another by some form of action. Four-year-old children used more verbs and prepositions in describing the "unitized" scenes, while 3-year olds' descriptions of related and nonrelated displays were not different. Nevertheless, the increase in recall produced by unitizing was equal for both age groups. Next, organizing letter strings into groups elicits chunking strategies, and the strategies of 3rd-graders resemble

those of adults (175). Finally, when presented with paired-associates lists containing response words that form an oppositional semantic dimension, both 5th-graders and college students learn the extreme words first, even though these are presented in changing random order (94).

Although Kendler, Kendler & Marken (193) reported that 4-year-olds did *not* employ categorical organization in a sorting task, as indexed by shift performance, there is evidence that young children can and will cluster words in free recall. Steinmetz & Battig (347) presented 4- to 13-year-old children with lists in which half the words were both highly familiar and strongly associated (e.g. popcorn, candy, peanuts). Marked clustering was noted in the recall of the youngest subjects and the degree of this clustering was equal across age groups. In addition, the tendency to report first the words that had just been learned (and presumably were most fragile) did not increase with age. Horowitz (161) also obtained an equal degree of clustering in the free recall of kindergarten and 3rd-grade subjects, and Nelson (270) reported that the categorical organization of 5- and 8-year-olds was equal even though the older children recalled more items per category. Comparing retarded and normal children matched on MA, Fagan (117) found large effects of IQ on free recall, but no effects on serial positioning and clustering. Apparently the poorer recall by retardates was not due to an inability to organize the items. Overall then it has been demonstrated that young children are sensitive to structure deriving from unification, spatial grouping, and the semantic properties of words. Whether the use of these structures by young children is as consistent and flexible as among older children, however, is a question for further research.

Evidence continues to support the notion that different mechanisms underlie retention and learning. Olton (277) found that a mnemonic provided by the experimenter increased the rate of learning among 5th-graders, but when presented as a cue for recall one week later, it had no effect on retention. In an extensive review of the literature, Belmont & Butterfield (25) concluded that, unlike acquisition, rate of forgetting in short-term memory is independent of the subject's use of organizational strategies. When differences in level of acquisition are equated, different age and IQ groups forget at the same rate.

It has been hypothesized that the acquisition of an S-R association establishes the backward R-S connection by a process of incidental learning. Since there are large changes with age in incidental learning, Kausler & Gotway (190) trained children from kindergarten to 6th grade on certain S-R pairs and then examined transfer to the R-S orderings. Contrary to the prediction, transfer was found to be equal across ages. Borkowski, McGrath & Doyle (35) obtained the same results by comparing retarded children and subjects with normal IQs. Thus, the mechanism underlying the learning of backward associations is apparently independent of both age and intelligence level.

The hypothesis that preschoolers use an iconic mode of representing

information while older children employ a symbolic mode as well led Corsini et al (74) to compare recognition memory for objects and for names of objects. Memory for objects was superior in preschool children, supporting the notion that iconic representation is primary at this age level. However, by 8 1/2 years, according to Davies (90), pictures of objects and their names are recognized equally well. This implies an increase in the use of the symbolic mode. The finding that children do better when presentation and recognition stimuli are in the same mode (either verbal or pictorial) suggests that these subjects stored the two types of stimuli in different modes.

Other evidence that a nonverbal mode of representation is used by preschool children was also supplied by Corsini (73). Retention of motor activities was better for such children when visual information supplemented the verbal instructions. On the other hand, adding the visual cues did not improve retention among 2nd-grade subjects. Since recall by the two age groups was equal when nonverbal cues were presented, the young child apparently represents information in ways which are qualitatively different from those used by the older child even though the representation may be equally effective.

Negative evidence concerning the hypothesized shift away from primarily iconic toward primarily symbolic representation was obtained by Poteat & Kasschau (288). No improvement in recognition memory for words was found between the ages of 7 and 11. Presentation of words that were either semantically or acoustically related to the stimulus word produced no decrease in acoustic errors nor increase in semantic errors with age, as would be expected if subjects were switching from acoustic to symbolic representations. For all ages, however, acoustic errors were the most frequent, although associative errors did occur. Whether or not modes of representation change with age, the evidence suggests that recognition memory itself is constant across age and is remarkably good in relatively young children. For example, 1st- and 4th-graders appear to be near perfect at picking out new words in a recognition memory task, although the younger children are poorer at discriminating the stimulus words from others they have previously free-associated to them (Hall 149).

Developmental changes in memory for visual and auditory material were reported by Stevenson & Siegel (349). School children were exposed to a film in which information was presented in two modalities. Recall of visual material increased from 3rd to 7th grade, but recall of auditory material declined at the later grades. Using an audiovisual analog of a dichotic listening task, Senf (321) also found an improvement with age in recall of visual items, while auditory recall generally remained constant for subjects from 8 to 15 years of age.

It has been suggested that the primacy effect (i.e. the tendency for items appearing first in a sequence to be better recalled) occurs because early items are rehearsed more thoroughly. Young children are particularly appropriate subjects for testing this hypothesis since they generally do not

rehearse. Earlier work has now been replicated, showing no primacy effect in the short-term recall of 4-year-olds (Kingsley & Hagen 195). As predicted, recall for early items was facilitated when these preschoolers were instructed to label pictorial stimuli and to rehearse the labels overtly. On the other hand, Donaldson & Strang (98) found that primacy effects occur in the recall of such young children, provided their strong position biases are taken into account. Whether this finding contradicts previous results is unclear, since no evidence concerning the presence or absence of spontaneous rehearsal was reported.

Cognition and long-term memory.—Inhelder (173) reviewed Piaget's surprising finding that memory improves over a six-month retention interval for a variety of displays, including serially ordered arrays of sticks. Improvement in recall of these arrays was replicated in two investigations (10, 86). In these studies, control subjects who were presented with an unordered array or with no array at all showed little serial production 6 months later, thus ruling out the possibility that Piaget's results derived from an age-related increase in spontaneous serial ordering. The basis for these changes is still unknown, although the Piagetian analysis emphasizes changes in cognitive structure during the retention interval.

Reinforcement effects.—A number of empirical attempts are made each year to establish that one stimulus is a better reinforcer than some other one. Few stimulus events, however, possess universal reinforcing value. On the contrary, the effects of a stimulus vary from subject to subject according to the vicissitudes of culture and the individual's life history. Therefore, many studies of reinforcement effects constitute little more than descriptive statements about an individual subject or sample; that is, the data offer information about populations rather than about psychological processes.

It has been reported, for example, that: (a) short exposures of a cartoon, when presented in the proper sequence, are effective in producing schedule effects among middle-class preschool children (391); (b) monetary rewards work no better than informative feedback for retarded readers enrolled in a special language program (340); (c) verbal feedback is neither more nor less effective than marbles in enhancing the performance of middle-class children (168) or lower-class children (362);³ (d) verbal approval increases high-payoff choices in a probability learning task more than a green light does (333); (e) both the intonation and the content of verbal reinforcers have reinforcing value (342); (f) exposure to photos of liked peers seems to reinforce the performance of 9th-grade school children working on a moderately difficult size discrimination problem (225); (g) praise increases achievement expectancies more than telling the subject he is correct or criticizing him, while lack of feedback changes expectancies less than any of the feed-

³ Earlier research showed otherwise (353).

back conditions (345); and (*h*) subjects who manifest high need for approval persist particularly long in responses leading to criticism, thereby suggesting that "negative attention" is positively reinforcing (133). Each of these findings gives rise to interesting speculations, but mostly about the nature of living and learning in the middle classes. We cannot extract a great deal from these data by way of general information about incentive functioning in child behavior.

A particularly interesting study was completed by Mischel, Grusec & Masters (253), in which they examined the effects of anticipated temporal delay on the subjects' evaluation of tangible rewards and punishments. In four experiments, with both children and adults, increases in the length of anticipated delay of a positive reward precipitated decreased subjective evaluation of it. Length of delay had no effect on evaluations of punishments, with one important exception—any anticipated delay, however long, increased the aversiveness of the punishment for adults even though this was not true for children. This study makes a relatively clear statement about one determinant of shifts in the incentive value of a stimulus. It also generates some interesting developmental hypotheses, although these need further study because of a confounding in this study of procedural differences with age.

For some 15 years, J. L. Gewirtz has studied the reinforcing efficacy of social stimuli in relation to the child's previous experience with the stimulus (137). Most recently he has used a "satiation-and-recovery" paradigm in which subjects were exposed to the word "good" between 0 and 32 times. Subsequent performance with this reinforcer proved to be inversely related to these satiation levels, and the length of an interpolated recovery interval also affected performance. Specifically, the two manipulations worked against one another in affecting the reinforcing efficacy of the social stimulus (136). Kozma (202) used the reverse research strategy, an isolation paradigm, to study the same problem and found that social isolation periods ranging from 0 to 18 minutes increased the effectiveness of verbal approval, but only for subjects scoring low on the *Test Anxiety Scale*.

McArthur & Zigler (238) have questioned whether the subject's relative satiation with the stimulus is the operative process in these studies. They have argued that the "valence" of the stimulus may be more important, and their results indicate that approval from "nice" adults is more reinforcing than approval from "mean" ones. On the other hand, relative satiation with the reinforcer had little effect. Actually, it is not inconsistent to argue that previous experience with *both* a particular stimulus class and a particular class of stimulating agent can affect the reinforcing efficacy of a social stimulus. Accordingly, the results cited here seem to us to complement one another and not to conflict. In fact, in one other study it was found that long-term deprivation in the form of institutionalization increased persistence on a socially reinforced task (Irons & Zigler 174).

Allen, Spear & Johnson (9) found an association between personality

characteristics of the experimenter and the effects of social reinforcement. The experimenter's "warmth" enhanced performance on a motor task, but only when the experimenter and the subject were of the opposite sex. Since earlier findings with respect to sex membership and experimenter characteristics have been confusing, the clear results of this study are refreshing. Grossman (144) investigated the relation between children's performance in a socially reinforced task and the nature of parent-child contact in the home. This study was small in scale and needs repeating, but the results are provocative. Parental warmth and interactions that fostered dependency were both positively correlated with task performance for girls but negatively correlated with performance for boys. Work of this kind provides a badly needed bridge between research on reinforcement effects and work on the childhood determinants of achievement behavior.

Punishment.—A substantial literature is rapidly accumulating concerning the effects of punishment on child behavior. Aronfreed (12) reviewed this material in an essay on the concept of internalization, and parametric studies of punishment were discussed by Parke (279). The newer research in this area emphasizes the interplay between various parameters of the aversive stimulus itself and the conceptual context in which the child perceives it. Willoughby's work on the effects of time out from positive reinforcement (388) is an interesting exception, but most of the research deals with cognitive structuring and its effects on various forms of aversive stimulation.

Information affects the value of punishment in numerous ways. First, the intensity of a punishing stimulus is related to behavioral suppression only when instructions are relatively nonspecific. Three studies (64, 65, 278) confirm this directly and one by inference (206). Next, only under nonspecific instructions does early punishment prove to be more effective than late punishment (Parke 278, Cheyne & Walters 65). Finally, the nurturance of the experimenter enhances the effectiveness of punishments but again, only under low cognitive structuring (Parke 278). Thus, explicit instructions seem to override the salience of numerous stimulus parameters in punishment training (see also Moore & Olson 257). Physiological data from at least one of these studies (64) also suggest that, under conditions of limited information about the procedures, anything which enhances the salience of the punishing stimulus increases its effectiveness.

In studies of discrimination learning, reward for correct responding plus punishment for incorrect responding usually produces faster mastery of the task than punishment alone or reward by itself. Sometimes punishment alone facilitates learning as effectively as the combination of reward plus punishment, but not always. Motivational interpretations have been offered to account for these findings, but an increasingly favored view emphasizes differences in information feedback provided by these conditions.

Following the informational hypothesis, Ahammer & Goulet (3) predicted that older elementary school children would deduce the meaning of "right" alone and "wrong" alone more easily than would younger children. That is, the older children were expected to show fewer differences associated with the feedback conditions than would the younger children. Instead, the usual trends were obtained in both groups: "right-wrong" feedback led to the fastest learning, followed by "wrong" alone, followed by "right" alone. Other investigators have shown that punishment consisting of the word "wrong" or a buzzer is superior, when used alone, to positive feedback used alone, but this superiority vanishes when the subject is fully informed about the reinforcement procedures (151, 152, 343). Consequently, punishment appears to work better than reward only when the situation is ambiguous or when the subject must formulate his own hypotheses about the meaning of the stimuli to which he is exposed. There is also indication that if rewards and punishments involve presentation or withdrawal of the *same* stimulus, punishment is not superior to reward in its effects on performance (Whitehurst 381).

Interruptions in rewards.—Individual differences were the focus of several studies of children's responses to delay of reward. Watson (374) found that intermittent reinforcement affected the proportion of children who learned a double alternation problem rather than their speed of learning. Success or failure pretraining, however, had the opposite effect, i.e. speed of learning was affected but not the number of subjects who learned the problem. Heilbrun (156) reported that perceived maternal nurturance and maternal control (attributes of parent-child interaction which should affect the individual's sensitivity to social reinforcement and his anticipation of it), were associated with performance under delayed reinforcement. Finally, Nakamura (266) found that "task persistence," as measured by teachers' ratings, affected performance under partial reinforcement along with changes in reward magnitude.

Partial reinforcement also produces interesting effects on behavior in interpersonal situations. Ryan & Strawbridge (310) observed pairs of 6-year-old children who were instructed to alternate at performing a lever-pulling task. In some dyads, one child was reinforced on a 50 percent schedule while his partner received 100 percent reward; in others, both children were continuously reinforced. Results were clearer for males, possibly because the competitive elements in the situation were more arousing for them. Generally speaking, however, responding was faster under intermittent reward whether the partial schedule was experienced directly or whether it was merely being observed. Responding was also faster when the subjects alternated on every other trial than when alternation occurred only after 40-trial blocks and when the instructions emphasized the personal responsibility of the subject for occurrences of nonreward.

Imitation.—Prominent among this year's studies of imitation is a group of investigations dealing with the functional role of the modeling stimulus. In addition, shifts have occurred in modeling theory, with a notable heightening of emphasis on the role of representational processes in observational learning. Several excellent reviews reflect these changes (e.g. Bandura 18, Aronfreed 13).

In two studies it was predicted that the subject's beliefs about the model's expertise would influence the amount of modeling (7, 218). Specifically, it was expected that models who were perceived as experienced would be imitated more readily than inexperienced ones. The results generally confirmed this. Nevertheless, conclusions concerning the effects of the model's background of experience must remain tentative because this variable was confounded in these studies with the presence-absence of an explicit performance norm. High experience was accompanied by specific information (e.g. "almost all children take tokens only for scores of 20") whereas inexperienced models furnished no norm at all.

In other work, Allen & Liebert (8) compared the relative influence of live models (who established a performance norm over a series of trials) to symbolic modeling (wherein the model simply told the subject about the norm he favored). The performing model was more effective in transmitting the norm but, as indicated, the number of exposures to the norm was more frequent in this condition than in the symbolic condition. Liebert and his associates (241) also have found that modeling is equivalent to direct instructions in transmitting self-reward norms, but the most interesting data from this research have to do with the effects of cognitive structuring (219). High structure ("20, that's a good score, that deserves a chip") produced greater adherence to a performance standard than did low structure ("20—take a chip"). In other research concerned with modeling cues, Fouts & Parton (126) found that exposure to objects being moved without a visible source of activation produced as much copying as did exposure to model-induced movements. Thus, for some types of observational learning, the presence of a human model is not necessary.

What functional role is played by the anguish of the victim in the modeling of aggression? Does it augment the effects of exposure to the aggressive acts or does it counteract them? Hartmann (155) found that a film which showed the bloody consequences of physical aggression enhanced aggressiveness in some subjects but inhibited aggression in others. All subjects who witnessed the model's aggressive acts were more aggressive than subjects who had not; observing the victim, however, increased aggression among those who had been experimentally aroused but decreased it among nonaroused subjects.

Three studies dealt with the role of mediational activity in observational learning; the results uniformly suggest that the efficacy of the modeling depends on the developmental status of the subject. For example, the modeling of mature information-processing strategies was more effective

among 5th- and 7th-grade children than among 3rd-graders (205). Specifically, the incidence of "constraint" questions in a game of *Twenty Questions* was increased when constraint questions were modeled; but only among the older children. In other work, assistance in verbalizing about the behavior of the model increased observational learning among preschool children but had no effect on 6- to 8-year-old children (68). Seemingly, this type of mediational assistance was superfluous for the older subjects. Finally, it was reported (365) that 7- and 8-year-old boys who could successfully perform a reversal shift profited from verbalizations about the model, while those who failed to perform the shift problem did not.

Imitative behavior appears to acquire some of its value as a consequence of the model's actions being perceived in conjunction with the experience of pleasant affect (Paskal 282). At any rate, by the time the child is of preschool age, successful matching to a standard seems to serve as a reinforcing stimulus (Parton & Fouts 280). Other investigators, still feeling it necessary to show that modeling "works," focused on self-reward behavior (157), self-criticism (354), increased social participation in the nursery school (274), and affectionate behavior (130).

SOCIAL AND PERSONALITY DEVELOPMENT

Concerns about "relevance" have led to increased stress on naturalistic methods of research. A volume edited by Willems & Raush (385) makes clear that increased attention to naturalistic strategies need not imply an anti-experimental bias, but this collection of papers reflects an increasing consensus that behavior oftentimes needs to be examined in the absence of experimenter intrusion. Moreover, certain aspects of development (e.g. attachment of the young child to his mother) cannot be conceptualized adequately unless information is available about contingencies that occur in natural child-rearing settings (Gewirtz & Gewirtz 135).

New methods for studying behavior in natural settings were described in several publications. Caldwell (50) presented APPROACH, an observational scheme for recording the social behavior of children in units that are easily prepared for computer analysis; Bijou et al (28) published a concise manual for conducting behavior modification studies in homes, schools, and institutions; and Bishop's well-known observational procedures (32) were applied to the study of status differentiation, affectional exchange, and emotional involvement in both mother-child and child-child interaction (Kogan, Wimberger & Bobbit 198, Olpin & Kogan 276). The *Ittleson Family Interaction Scales* were published (23) and, like the prototypic *Fels Behavior Rating Scales*, were designed for observational study of family interaction in the home. These improvements contrast sharply with several unsuccessful efforts at improving interview methods (Chamberlin 61, Cotler & Shoemaker 75).

Peer relations and social attitudes.—Some of the richest naturalistic data

in the social psychology of childhood are contained in the work of Muzafer and Carolyn Sherif. Their most recent studies (328) concern the functioning of spontaneously formed adolescent reference groups, with special emphasis on the role of norms and status differences in the activities of such groups.

Other investigators still prefer to study group phenomena in the laboratory, and a wide variety of methods, long popular in studies of adult groups, have been adapted for use with children. Much of the research thus far completed with paradigms such as the *Prisoner's Dilemma Game* and the *Maximizing Difference Game* has been descriptive, and relatively little is known about the general potential of these devices for studying problems in developmental psychology. It has been found rather consistently, however, that females are more accomodative and cooperative in their game strategies than males, and that cooperative strategies decrease with age (240, 325, 338, 352).

Cultural differences have been explored using both Madsen's *Cooperation Board* and the *Prisoner's Dilemma Game*. Data are inconsistent concerning the characteristic response strategies of groups of Negro and Caucasian children on these tasks (26, 271), but it has been reported that, under competition-inducing conditions, cooperation is better maintained among kibbutz-reared than among city-reared Israeli children (324). It strikes us that these paradigms should now be applied to less global problems, perhaps following the strategy used in studies of group problem solving. Kogan & Carlson (199), for example, found that group support produced smaller increases in individual goal setting among 4th- and 5th-graders than it did among college students. Such findings raise interesting developmental questions.

Methodological improvements are also occurring in research on interpersonal attraction and social distance. Guardo (146) elicited degree-of-liking designations by obtaining reactions to silhouettes spaced by the examiner at varying distances on sheets of paper. The validity of these "personal space" measures is indicated by the inverse relation obtained between interfigure distance and degree of designated acquaintance and liking. Also, girls as contrasted with boys use more interpersonal space in situations involving negative affect, and figure placement of same-sex peer silhouettes is closer among elementary school children than among secondary school pupils (247). Although scarcely surprising, these findings indicate the potential value of this method.

Perhaps the most interesting new data on social attraction were published by Albert and Bernice Lott. Theirs is a reinforcement theory of attraction, and over a period of years this research team has produced a number of interesting experimental paradigms. Now, for example, they have found that game players who received direct reward and their helpers (who received no reward themselves) both developed more positive attitudes toward a work group than did nonrewarded subjects or their helpers (226). In another study, adult experimenters who were present while the child per-

formed an immediately reinforced response were better liked subsequently than experimenters associated with a delayed reward (224).

From two different sources comes information that restricted social experience reduces the linguistic richness of children's descriptions of social roles. Isolated communal living was found to be associated with reduced cognitive complexity in such descriptions (318), and students attending a small high school used a wider range of interpersonal constructs than did matched subjects who attended a larger, more socially diffuse school (383). Whether range and frequency of social experiences or the intimacy of them produced these results is not clear, but the findings indicate linkages between the child's social experiences and his linguistic development. Another linkage between social and cognitive development is represented by the increase with age, found by Atwood (14), in the accuracy of children's inferences about people based on what the subject knows about the person's relations with others.

On the basis of Piaget's notions concerning the development of moral judgment, the prediction can be made that social conformity should increase during middle childhood and decline with the onset of adolescence. Earlier evidence generally supports this view in that the shape of the age gradient for many different types of conformity behavior appears to be nonlinear. Even hypnotic susceptibility shows this trend (London & Cooper 223). But the peaking of conformity in middle childhood varies according to the demands of the situation and the nature of the conformity task. Hoving and associates (153, 165) showed that the middle childhood peak is more evident when the conformity task is partly ambiguous than when it is difficult or very easy, although the adolescent decline was not well documented since the oldest subjects in these studies were only 13. Utech & Hoving (363) found that parental influences in a conformity task become less salient, relative to peer influences, during middle childhood, but other studies continue to show that the relative influence of peers and parents depends on the social norm in question (Kandel & Lesser 185, Brittain 40).

One year's work is a very small sampling of the literature dealing with the development of social stereotypes, and there was no surge of effort in this particular area. Stereotypic notions about the relations between body build and behavior are found as early as the 10th year (Lerner 209) and consistent aversion to "chubbiness" has been reported in the social preferences of kindergarten children (Lerner & Gellert 210).

Studies of attitude change as a function of school integration are not as comprehensive as they should be, considering the magnitude of this social problem. One small but interesting study concerned the impact of multi-ethnic readers (Litcher & Johnson 220). White 2nd-grade children in a mid-western city were given these curriculum materials with no special discussion or instructions; experimental controls were exercised for teacher, classroom, school, and reading ability of the subjects. Four months' use of the readers resulted in significant increases in positive attitudes toward Negroes,

supporting the view that curriculum materials can function to counter-condition racial stereotypes. Less direct approaches to this problem, such as altering children's general associations to the color "black" (387), strike us as inefficient solutions to racial problems.

Research on the self-concept occupies a rather peculiar position in developmental psychology. Notions about the "self" figure prominently in many theories of personality and social development, yet empirical studies have always been few in number. This past year was no exception. One developmental study was published, showing a decline in self-evaluation by males from ages 8 through 12 (181). Otherwise, it was learned that deaf children with deaf parents made more positive self-evaluations than deaf children with hearing parents (243), and it was confirmed that minority group self-concepts are less positive than those of majority group members, and achievement expectations are more discrepant from actual achievement (60, 147, 236). Anxiety and its relation to self-evaluation was the focus of two studies: (a) for boys, but not girls, anxiety was negatively related to self-concept, while self-attitudes were positively related to performance on a verbal learning task (Lekarczyk & Hill 208); (b) greatest anxiety was found among children who thought poorly of themselves *and* who were evaluated negatively by their peers (Felker 120). In general, these results cement conclusions from earlier research.

Affiliation.—Studies of dependency seem to be waning in numbers, although considerable work is still being done on need for approval. Eininger & Hill (108) found that among preschool-aged girls, giving affection to others was positively correlated with the seeking of it, while the giving and seeking of help were not related. Emphasizing that approval-seeking may be a more appropriate trait for girls than for boys, Tulkin, Muller & Conn (361) found a positive correlation between scores on the *Children's Social Desirability Scale* and popularity for girls, but a negative relation for boys. Dodge & Muench (97) reported no demonstrable relation between need for approval and conformity in children (contrary to findings for adults), suggesting that the socialization of need for approval may have a rather complicated developmental history.

Cultural norms have considerable influence on need for approval. In at least two different settings, children of strongly religious parents had higher social desirability scores than did children who had parents of lesser religiosity (78). Among other child-rearing studies, however, one finds a common theme: an affiliative, person-oriented disposition is the outcome of nurturant attitudes and acceptance of the child by his parents. This pattern was found with the following dependent variables: (a) willingness to disclose things about oneself to others (99); (b) expectations that the words, promises, and statements of other people are creditable (189); (c) susceptibility to influence by both adults and peers (311); and (d) attention to, seeking information from, and compliance with maternal requests (41). Although

none of the foregoing studies is strongly convincing by itself, the weight of the total evidence is substantial. Even some odd findings by Medvene (244) do not seem serendipitous: among male psychology majors, those interested in person-oriented careers perceived their fathers as dominant-accepting or dominant-attentive, while those interested in careers not directly related to people perceived their mothers as dominant-avoidant!

Each year brings a spate of studies concerning the relation between birth order and affiliative behavior. Most of these investigations are performed with adult subjects and most of the new studies support Schachter's well-known theory. For example, stronger affiliative orientations were found among first borns than among later borns, as measured by: (a) voluntary participation in shock experiments (235); (b) fear and pain sensitivity in the dentist's chair (91); (c) church attendance (234); (d) self-rated anxiety prior to a stress experience (299); (e) high representation in a counseling program for superior students (38); and (f) attitudes indicating high anchorage in some identifiable social system (196). One study reported negative findings—i.e. no relation between birth order and field dependence (83). In spite of the clear pattern in these results, however, interpretation is often difficult because of design problems. Attempts at isolating some of the sources of bias in birth order studies are rare, however, so it is encouraging to note the publication of two excellent methodological papers by Hare & Price (154, 289) and a careful review of sibling-order studies by Sutton-Smith & Rosenberg (350).

Moral behavior.—It has long been held that there is little trans-situational generality in children's cheating. Since the empirical support for this notion is over 40 years old, Nelsen, Grinder & Mutterer (269) studied cheating in a group of Wisconsin 6th-graders in six different temptation situations. Only modest consistency in cheating was found, providing little basis for questioning earlier conclusions. Johnson & Kalafat (180) also reported low inter-correlations among three projective measures of resistance to temptation and zero-order relations among a small group of guilt measures.

An altogether different approach to moral behavior is represented by the studies of Hill and his associates. These deal with the hypothesis that cheating is an attempt to avoid devaluation by self or by others. In one study of 6th-grade boys (159), the most cheating occurred among those who both knew that their peers had succeeded at the task and believed that they themselves were safe from detection. Lesser cheating was manifested in "no knowledge" and "high risk" conditions. In a second study (326), anxiety was positively correlated with cheating, but only among subjects who knew their peers had succeeded at the task. Ross & Ross (303) have shown that fear of aversive consequences is also involved in post-cheating behavior. Preschool children who cheated on a maze task later espoused more lenient rules in a group of story completions than did those who broke no rules on the task or children who were not given the maze problem. Thus, fear of

negative consequences appears to determine behavior following deviation—when deviation occurs—as well as to instigate the cheating itself.

How do social experiences affect the child's conception of moral justice? An older study by Bandura & McDonald (19) showed that modeling induces both progressive and regressive changes in the maturity of children's moral judgments. These changes were measured immediately after a brief modeling session and with a relatively restricted range of judgments. Nevertheless, the authors felt that their data presented a serious challenge to "stage-invariant" theories of moral development, e.g. Kohlberg (197). This conclusion has been resisted by several investigators because newer data suggest that progressive changes—those involving a shift to a slightly more mature level—are easier to induce and last longer than regressive changes (Rest et al 293, LeFurgy & Woloshin 207). It is argued that brief exposure to models must produce only a transitory "conformity-type" of change and not a basic shift in the child's moral orientation.

Cowan et al (76) completed a careful replication of the Bandura & McDonald study, lengthening the time between modeling and post-test, and using a wider sampling of moral judgment items. The results confirm that modeling produces both progressive and regressive changes. These authors, however, also argue that the data are not germane to stage theories.

The controversy then concerns the meaning of the data. Cowan et al argued that modeling studies provide data on limited aspects of moral reasoning, overlooking several dimensions of moral judgment, as well as intention consequences, which would be assessed by Piaget. Pleas that Piaget's theory of moral development has been misunderstood or inappropriately assessed in these studies were also entered by Lickona (217) and by Rest et al. Such arguments, however, strike us as specious. They are reminiscent of the attacks made by orthodox psychoanalysts on the so-called "objective studies" of psychoanalytic phenomena. Furthermore, the facts remain. Modeling is implicated as a *process* in the development of children's moral thinking even though the *extent* to which modeling is involved in the development of moral judgment and the nature of its interaction with other aspects of the child's cognitive functioning are less clear. The importance of these interactions is recognized by many social learning theorists, including Bandura (17), and therefore one wonders about the point of this controversy.

Sex role behavior.—Recent work on sex-typing indicates that this area is in need of conceptual retooling. Mussen (263) analyzed the literature, and Simon & Gagnon (335) discussed sexual development as "a socio-cultural invention . . . that in itself explains little and requires much explanation." Lynn (230, 231) attempted a theoretical analysis of the major sex differences in children's development, stressing the interplay between social learning influences and certain aspects of cognitive functioning. This theory, however, is somewhat sketchy and consists mainly of a listing of empirical findings.

Several bits of information emerged concerning the importance of family composition and dominance patterns in sex-role development. Biller (29) confirmed that the father's presence in the home is a significant factor in the development of masculine attitudes in young boys. He also reported that among father-absent boys, maternal encouragement for masculinity was positively related to the appropriateness of the child's sex-role orientation. In other work, the dominance of the father, particularly as perceived by the child, was found to be positively correlated with masculinity in the personal orientations and social perceptions of boys (Biller 30, Rutherford 309). When the father's presence is taken for granted, however, women seem to relax their strong encouragement of masculinity. An *in vivo* analysis of reinforcing contingencies in the nursery school (118) showed that peer interaction supported appropriate sex-role behaviors in both sexes, but the female teachers tended to reinforce a preponderance of feminine behaviors regardless of the child's sex.

Aggression.—Current studies of the childhood determinants of aggression are not extensive. The report of a presidential commission, *Violence and the Media* (16), indicates that child psychologists possess much information concerning the determinants of aggression but very little stemming directly from naturalistic sources. Perhaps the most interesting new material is contained in a pair of studies that emphasize sex differences. Feshbach & Feshbach (122) found that empathic tendencies were positively associated with aggressiveness in preschool-aged boys, but negatively correlated among somewhat older boys. On the other hand, no relation was found between empathy and aggression in girls. Feshbach (121) found that boys and girls express direct aggression toward new members of small groups in equal amounts, but girls show significantly more indirect aggression. Observation of such groups showed, however, that this sex difference was apparent only in the children's *initial* responses to outsiders and that the later interactions of the two sexes did not differ.

Achievement motivation and behavior.—It is increasingly clear that projective measures of achievement motivation are erratic predictors of achievement behavior in specific situations such as the school. Khan (194) generated multiple correlations ranging from .48 to .69 with measures of study habits, attitudes, motivation, and anxiety as predictors. These coefficients represented some improvement in the predictions that could be made about the achievement of these junior high school subjects solely from IQ. The fact remains, however, that motivational measures do not account for much of the variance in academic achievement.

The effects of various adult reactions on children's achievement expectations were examined in four separate instances: (a) underachievers in school responded to *nonreaction* as if it were positive reinforcement, while achievers responded as if the *nonreaction* were negative reinforcement (2); (b) among

children younger than those studied previously, shifts from praise or criticism to nonreaction produced little effect on performance, but shifts from praise to criticism (or the reverse) generated large changes in performance (258); (c) initial achievement expectations were negatively correlated with test anxiety among girls, but changes in expectancies after receiving social reinforcement were positively correlated with anxiety for boys (160); (d) telling the subject that he "missed" on incorrect trials produced no change in 2nd-graders' level of aspiration, but withdrawing a token did (371). Thus, the effects of reinforcement on achievement expectations depend on many factors; each now requires more intensive study across ages and personality types than has been given thus far.

Gender effects in studies of this type are suggested by the results of both Walberg (367) and Stein & Smithells (346). Children appear to rate performance as masculine or feminine depending on the kind of activity that is involved, and moreover this differentiation is more pronounced among adolescents than among grade-school children.

The children studied by Sears, Maccoby & Levin in 1952 were studied again, this time with respect to aspiration level. High goal setting at 18 years of age was correlated with early maternal punitiveness toward aggression and dependency and with high maternal anxiety (Crowne et al 82). When these findings are combined with other new data, it appears that adolescent achievement aspirations may have different antecedents than does early achievement behavior, which appears to be more closely related to demands for assertive autonomy (267).

Some half-dozen studies indicate that present-day schools foment alienation, detachment, and anxiety in their pupils and that these attitudes develop both independently and concomitantly with academic failure. The major factor differentiating graduates in the New Haven public schools from dropouts was "adaptation to middle-class values" (Namenwirth 268). Interviews with Chicago school pupils during vacation periods showed increasing sensitivity to competition and failure between the 2nd and 6th grades (129), and Ohio adolescents expressed similar attitudes (182); also, underachievers are more passively aggressive than comparable children who are achieving appropriately (260). Students who perform poorly in high school can be identified as early as the 3rd grade, although dropouts begin performing poorly at a somewhat later point than do those who stumble on through to graduation (125). Lastly, a demographic study showed that Tennessee children who start school as the youngest in their class are more likely to be identified later as maladjusted (377).

Extraverted children do better in school than introverted pupils, according to data from large-scale testing by Eysenck & Cookson (115), while degree of neuroticism seems to act as a moderator variable in this relation. Although genetic factors account for as much as half of the within-family variance in introversion-extraversion, data from both longitudinal studies and studies of twins point to inheritance-environmental interaction in the

ontogenesis of this orientation (Scarr 314). Bronson (43) also advanced the tentative hypothesis that cultural influences, including those of peers and school, act in such a way as to support early temperamental orientations. Thus, from direct work on school failure to correlational data on temperament, findings concerning individual differences and the child's school environment present numerous depressing aspects.

Creativity.—Studies of creativity fall into three main groups: situational factors in assessment, personality correlates, and childrearing antecedents. Kogan & Morgan (200) published the results of an extensive study that furnishes no clear support for the use of either test or game-like contexts in assessing creativity. Scores depend on the task, the measure of creativity, the anxiety level of the subject, and sex. Ward (369), however, found that the associative responses of nursery school children were increased by the perceptual richness of the testing room. This occurred only among relatively creative children, indicating that creativity may involve a special propensity to scan the environment for task-relevant information.

Correlational analyses, based on several different populations, showed the following: (a) subjects producing more total ideas in a fluency task give them at a faster rate, although response fluencies are not associated with proportion of uncommon responses (370); (b) mixed sex-role patterns are associated with greater fluency and uniqueness of response on creativity tests than are consistent sex-role patterns (31); (c) creative adolescents in the literary field more often report having had imaginary companions in early childhood than do matched controls (315); (d) the self-concepts of creative adolescents include reference to complexity, impulsivity, need for novelty, autonomy, and self-assertiveness (316). Thus, the creative child is depicted as environmentally alert, autonomous, and somewhat offbeat, although this group of studies is too small to more than sketch the creative "character."

Cognitive style.—Cognitive style research touches upon many developmental problems, including attentional mechanisms. Siegelman (331) directly studied the manner in which observing behavior was deployed in a match-to-sample task. She found clear differences in the distribution of attention between children identified on the *Matching Familiar Figures Test* as impulsives and as reflectives. The latter looked longer at the problem material, but they also looked proportionately less at the important stimulus members involved in the problem, i.e. the standard, the alternative finally chosen, and the most observed alternative. In contrast, the impulsives showed greater inequality within the range of stimuli to which they attended. Meichenbaum & Goodman (246) offered preliminary evidence that reflectivity-impulsivity is related to the verbal control of motor behavior. On a pushing task of the type invented by Luria, impulsive children showed less verbal control over their motor behavior than the reflectives when

verbalizing was covert. This difference, however, disappeared during overt verbalizing. Achenbach (1) studied the relation between associative responding and cognitive style, finding that impulsives were more likely than reflectives to choose the foil in a multiple choice test containing a strong "associate" to certain words included in the question stem.

One training study appeared (Baird & Bee 15), demonstrating the feasibility of a learning analysis of cognitive style development. In this study, analytic and nonanalytic responders were given training *opposite* to their basal preferences. Shifts occurred in both groups during training, but an immediate post-test showed that these shifts were greater among subjects trained in analytic as opposed to nonanalytic responding. Consequently, it appears that training can be accomplished more readily in a progressive, mature direction than in the reverse direction, indicating that the thrust of development modulates the effects of contingent reward on conceptual style.

Three cross-cultural studies revealed differences between rural and urban populations. In Okonji's study (275), rural-urban differences were confounded with differences in literacy, so that it is difficult to evaluate his report that city youth were more field-dependent than rural youth. Clearer data emerged from two other subcultural comparisons. Maccoby & Modiano (233) found that characteristic conceptual responding among Mexican peasant children included emphasizing differences rather than equivalences, moral realism, and concrete perceptual attributes; urban children, on the other hand, made greater use of abstractions, equivalences, and relativistic moral judgments. Differences in coping style were also found between a sample of urban Mexican children and a sample from the United States (Holtzman et al 164). These milieu effects on conceptual style probably have important implications for social planning in societies in which industrialization is occurring, although such cultural changes are not always accompanied by difficulty, either for parents or for their children (Mussen & Beytagh 264).

FINAL COMMENT

Appended to this chapter is a reference list ten times as long as the one which accompanied the 1950 *Annual Review* essay on developmental psychology. Our list represents but a fragment of the current work, although a large number of trends have been depicted in our text. Topical trends, such as the "cognizing" of virtually every subsection of the field, are clearly evident. But it is also clear that most of the enormous output in developmental psychology is guided by the aging theories of the past or by no theory at all. There is redundancy in this field as well as great activity. The problem for the future then is to produce those giant theoretical steps that are needed to establish new directions for an already vigorous and highly visible field.

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PERSONALITY

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Reviewing a body of literature is to some extent a concept formation task—or at least it should be. Too often, as with not a few term papers turned in by students, it is an annotated bibliography. To avoid doing this in the area of personality is an especially challenging task since its tentacles extend into so many parts of the study of behavior. Writing an annual review compounds the challenge because of the danger of losing sight of the forest's outline when entangled in clumps of trees within its borders.

SITUATIONAL VS DISPOSITIONAL VARIABLES?

In recent years certain personality psychologists (e.g. 210, 211, 232) have emphasized the roles of situational factors and environmental contingencies while at the same time either ignoring, dismissing as unimportant, or de-emphasizing the contribution of dispositional variables—such as attitudes and inferred motivations—to the understanding of behavior. There can be no quarrel with the position that stimulus factors are powerful determinants of behavior. However, we do question the scientific wisdom of simply relegating, as it were, individual differences to the “error term.” Their systematic inclusion in research designs allows not only the study of situational variables, but also assessment of the manner in which individual differences serve to moderate S-R relationships.

Every study in which significant interactions emerge between personality and situational variables (and we shall cite a goodly number of these) underlines the fallaciousness of attempts to understand behavior without considering what individuals bring with them to laboratory, clinical, and everyday life situations. Implicit in the scientific enterprise is the aim of reducing the “error term” through specification of the relative contribution of variables influencing the phenomenon under study. In the case of personality research, this would seem to entail research designs in which the behavioral effects of environmental and individual difference variables, and their interactions, are concurrently studied. An example of the way in which assessed and manipulated variables interact is seen in an experiment reported

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by Jacobson, Berger & Millham (152). They explored the relationships of inferred self-satisfaction, social desirability, and need for social approval to cheating when confronted with failure. Two groups cheated significantly more than did the other groups: women scoring high on the self-satisfaction measure, and subjects scoring high simultaneously on both self-satisfaction and need for approval. In another study, cheating was found to be positively correlated with achievement anxiety, but only when knowledge of reference-group performance was provided (263).

We are not arguing that individual difference variables are more important (i.e. account for more variance) than environmental variables; indeed, the opposite is probably more often the case. What we do maintain is that dismissing individual differences in personality characteristics because of the difficulties in measuring them or the procedural problems involved in incorporating them into research designs serves to vitiate the avowed goal of understanding human behavior. Certainly the restriction of scientific endeavors simply to the study of functional relationships between environmental variables and behavior seems at first glance to be consistent with the physical sciences paradigm after which many psychologists strive to model their science. But is it really? Our contention in this regard has been expressed well by Eysenck:

No physicist would dream of assessing the electric conductivity, or the magnetic properties, or the heat-resisting qualities of matter, of "stuff-in-general" . . . Much energy was spent on the construction of Mendeleev's tables of the elements precisely because one element does not behave like another. Some conduct electricity, others do not, or do so only poorly; we do not throw all these differences into some gigantic error term, and deal only with the average of all substances. But this is what experimental psychologists do, in the cause of imitating physics! It may be suggested that the root of many of the difficulties and disappointments found in psychological research, as well as well-known difficulties in duplicating results from one study to another, may lie in this neglect of individual differences (101, p. 5).

Two especially noteworthy 1969 publications dealing with this neglect were Vale & Vale's article (296) and Carson's book (61). The Vales point out the generality—for all of psychology—of the topic of organism-environment interactions. They argue that general laws of psychology require systematic inclusion of organismic variables in experimental investigations. Carson stresses an interpersonal approach to behavior and the need for balance in assessing these interactions. He points out that inner propensities are of little value in the absence of linkages to actual environmental conditions and requirements. While not a few theorists (47, 66, 284) have taken paths toward individuality that at least to some extent eschew a purely data-oriented attitude, the empirical approach to personality dominates. This approach leads to three paths. While discernible, they are in some places poorly maintained and neglected; in other places peripheral trails link them to one another.

One path has been taken by those whose preoccupations are with the determinants of individuality. Among these determinants are the individual's history of experiences, his genetic heritage, and a host of biophysical factors ranging from whether or not he has a physical handicap to his basal metabolic rate. Those who focus on the determinants of behavior seek the causes of present attitudes and reactions to others, to stress, and to the behavior of everyday life. The decision to engage in this pursuit does not necessarily require any particular theoretical view of the nature of man, but once committed to the pursuit, theoretical predilections exert powerful influences. Which determinants of which behaviors are pursued? Personality theories have traditionally placed the spotlight on interpersonal processes, our relationships with others, and developmental theories, such as psychoanalysis, have stressed the significance of the early years of life (9, 10). Occasionally, social and Zeitgeist forces intrude on cloistered theory-building activities by providing challenges and opportunities, if we but note them. Psychological theory did not much influence the Israelis' confident invention of the kibbutz or the United States' hesitant Headstart steps. Yet developments such as these have, or more precisely, can have, profound theoretical implications. Personality theory not only must be modern in its methods, it must be responsive to changes in social institutions and mores as well (46, 86, 207). Campbell (54) has discussed ways in which social reforms may be conceptualized and analyzed as experiments.

A second path to personality explores not so much the determinants of individual differences as the individual differences themselves. A number of human characteristics are relatively stable over long periods of time (height, weight, etc.). Others can show either short or long-term changes, as when normal psychomotor ability is impaired by a physical disability. The psychologist who seeks to uncover the cause of personal characteristics and the psychologist who specializes in individual differences themselves have much in common, notably the use of empirical methods and an interest in prediction. But the methods they use frequently are different, with the latter emphasizing correlational techniques and the former adhering to an experimental approach. The correlational researcher builds his armamentarium of tests and measures on the reasonable assumption that the present and the past provide valuable clues to the future. But the decision to use personal characteristics in order to predict behavior in no way tells us *which* characteristics are the most predictive of which *behaviors*. Thus, students of personality who adhere to assessment and experimental methodologies find themselves in the same boat. They face the same question: In which of many directions should they sail so as to reach the most productive shore?

One of the significant developments in personality research during the past two decades has been the exploration of a third path that incorporates *both* individual differences and experimentally manipulated variables. The aim of researchers who use this approach is to increase the meaningfulness

of experimental results by applying treatments not to random samples, but to subjects who differ on theoretically relevant dimensions. The designs employed in this endeavor not only reduce error variance but also provide opportunities to demonstrate construct validity. Illustrative of the growing interest in establishing construct validity is research on stress. Prior to World War II, most studies that dealt with this topic involved simply the manipulation of particular experimental variables (physical conditions, time pressure, reproof). During the postwar period, studies of stress have more and more included individual difference variables that theoretically might be expected to interact with experimental treatments.

PERSONALITY ASSESSMENT

A problem for the researcher who takes this third path is to decide *which* of many individual difference variables to include in a given study. Assessment psychologists have concocted a voluminous literature concerning personality factors. By the standards of psychology the field of personality assessment is a venerable one. There are no signs that hardening of the arteries is setting in. Thus, Buros (48) has published a 1659 page compendium of current personality tests and measures. It contains test descriptions and reviews by experts in the field. The book will prove to be a primary reference source.

NEW SCALES

One index of the activity during the past year is the large number of new personality scales which have appeared. New measures have been developed to tap dimensions as diverse as empathy (144), social caution (95), suspiciousness (97), subjective moral attitudes (235), impulse control (173), defense mechanisms (110), social-evaluative anxiety (302), novelty seeking (231), fear of death (69), conservatism (309), and personality conflict (162).

The items of these scales are often borrowed from a variety of sources. For example, the Hogan (144) empathy scale contains CPI, MMPI, and IPAR items which were found to discriminate between subjects rated as empathic or nonempathic. In cross-validated samples, the scale was found to predict rated empathy better than other existing measures. Peer ratings, test correlates, and other evidence indicate that high scorers are socially acute and sensitive to the feelings of others, while low scorers are cold, hostile, and insensitive. The scale may be of special interest to researchers concerned with moral development.

An adequate measure of social-evaluative anxiety has been a long-unanswered need. A test developed by Watson & Friend (302) is composed of two scales: Social Avoidance and Distress (SAD), and Fear of Negative Evaluation (FNE). Validation data support the construct validity of the scales; high SAD scorers prefer to work alone, report less conversational behavior, and tend to avoid social situations. Subjects high in FNE become

anxious in evaluative situations and are highly motivated to gain approval and avoid disapproval. An advance in the theory and assessment of state and trait anxiety has been made by Spielberger and his colleagues. Spielberger, Lushene & McAdoo (273) have traced the theoretical development of the concept of anxiety and devised six separate self-report measures of state and trait anxiety (115, 272). Data on their use with junior and high school students, normal adults, and psychiatric, medical, and surgical patients are now available. The test manual is clearly written and includes more normative data than are usually presented for anxiety indices.

The Defense Mechanism Inventory, developed by Gleser & Ihilevich (110) is an ipsative paper and pencil measure dealing with five defensive modes: hostility-out; projection; principalization; turning against self; and reversal. Each mode incorporates one or several psychoanalytic defense mechanisms. The scale consists of ten stories followed by questions regarding the behavior, fantasies, thoughts, and feelings which the subject would have in the situation described. Test-retest reliability appears adequate and construct validity data suggest that the inventory has potential utility as a clinical and research tool.

The Situational Appraisal Inventory (235) was designed to provide a measure of strength of moral values and evaluative attitudes. In making their responses, subjects are asked to evaluate the degree of "wrongness" of a fictitious character's behavior in a series of situations in light of the circumstances under which a socially undesirable act takes place. Reliability of the inventory seems to be adequate. High scores are related to sex (females higher), religiosity, and, in females, strength of parental identification. Johnson & Kalafat (156) found that a projective measure of conscience development was unrelated to overt behavior as rated by peers. Whether or not amount and type of guilt is related to mental health remains an open question, but research interest in assessing moral attitudes and conscience development is decidedly on the increase (91, 264). Two problems that require careful inquiry are the relationships that exist among different measures of superego variables, and the relationship between these variables and theoretically significant facets of behavior.

RECENT DEVELOPMENTS

Advances on a theoretical level lag far behind the production of new scales. One consequence of this is that the area of assessment often appears chaotic and disjointed. Small groups of investigators interested in specific variables continue to "do their own thing" with their own measures. Very little effort is made to integrate within a comprehensive conceptual framework disparate findings regarding particular personality variables.

The pitfalls involved in attempting to assess significant personality attributes are many and varied, and the "true score" of an individual's standing on a given dimension is as elusive as the Holy Grail. After a test has been published or a new technique announced, it may continue to be used in

the absence of any convincing evidence in its favor. The knockout punch is apparently hard to land. For example, the already shaky status of the Draw-A-Person technique was further undermined by the results of a study by Wanderer (300), which found experts in the use of the technique unable to match drawings with diagnostic classification (normal, neurotic, mental defectives, homosexuals, and schizophrenics) except in the case of mental defectives. Hammer (129), while questioning the meaningfulness of Wanderer's results on a number of conceptual and methodological grounds, concedes that it raises some vexing issues regarding applications of the Draw-A-Person technique.

Response sets.—A prevalent issue in recent years has been the role of response sets such as acquiescence in the MMPI (105, 151). Bock, Dicken & Van Pelt (37) have concluded that acquiescence accounts for a relatively small portion of the variance as compared with content. They contend that the properties of MMPI scales attributed to acquiescence may be accounted for by the substantial correlation between acquiescence and the first factor content in the MMPI.

Edwards (94) has sought to integrate the complex methodologies involved in the measurement of personality traits. The results of a study by Herron (139) suggest that the response set to check many or few adjectives is a prime determinant of adjective check list affect scores. Goldberg & Jones (112) have raised a number of searching methodological questions concerning intraindividual consistency in responses to self-report questionnaires. Is this consistency a significant trait in and of itself? How homogeneous and stable are measures of response consistency? Do they have validity? A major stumbling block to answering these questions is that response consistency is confounded with variance attributable to the particular sets of items studied.

"Test-wiseness" has been a longtime concern of users of self-report personality scales. Stricker (281) defined this variable in terms of ability to (a) estimate social desirability and frequency of endorsement; (b) identify items measuring the same factor; and (c) change scores on personality scales when instructed to fake good or bad. He found test-wiseness measures to be uncorrelated with one another and to correlate only moderately and occasionally with personality measures. However, they consistently correlated with scales measuring social desirability response set. Braun & LaFaro (41, 42) found that instructions to fake "good" impressions affected 10 of the 16 scales on the 16 PF (Personality Factors) Questionnaire, but had little effect on the Personal Orientation Inventory.

Rating scales are among the war horses of personality research. The factor structure of these scales is often found to be invariant between and within individuals even when different objects are being rated (168). Kuusinen (169) suggested that this factorial invariance may be the result of in-

variance of the structure of affective meaning, as outlined in Osgood's theory of meaning (225). He found that partialing out from personality ratings a group of affective factors, defined on the basis of semantic differential scales, resulted in more meaningful personality dimension ratings. The effects on other types of personality measures which would result from partialing out affective meaning factor structures remain to be investigated.

Inferences concerning personality.—A continuing and tantalizing enigma for many personality researchers is the manner in which judges draw personality inferences from diagnostic test signs. On the basis of research findings, such "signs" often lack the psychological meanings which clinicians assign to them. A provocative line of research dealing with the meanings attached to diagnostic signs has been initiated by Chapman & Chapman (64, 65). It points to a significant source of systematic error which helps account for this phenomenon. These investigators term the tendency to perceive a relationship between a test sign and a behavior when the relationship is either weak, nonexistent, or in the opposite direction as "illusory correlation." The results of their initial (64) study with the DAP suggested that illusory correlation is mediated by the verbal associative connection between the sign and the attributed symptom. Their 1969 study (65) reported similar findings with the Rorschach and showed that clinical diagnosticians failed to report valid signs for male homosexuality although they reported observing invalid signs which have stronger rated verbal associative connections to male homosexuality than do the valid signs. Undergraduate college students "rediscovered" the same invalid signs as the clinicians reported observing in their clinical practice even when the valid signs were paired with a homosexual symptom statement more often than were the invalid signs. Starr & Katkin (276) have also reported illusory correlation effects with Rotter and Rafferty's Incomplete Sentences Blank. Their findings parallel those reported by the Chapmans with the Rorschach and figure drawings. The fact that illusory correlations have now been reported with three of the most frequently employed projective tests points to associatively mediated effects as powerful sources of the bias that leads to accepting invalid signs and ignoring valid signs.

The general problem of how men make judgments about others and the validity of such judgments remains unresolved. Goldberg (111) points to a consideration of the clinical judge's model of human behavior as a path toward its solution. He presents data indicating that the combined judgments of a group of clinicians, each possessing some degree of predictive know-how, may be the best predictors or classifiers of people's behavior. There is a great need for research on the question of how people acquire, process, and act upon subjective "knowledge" (221). In this regard Rosenthal (248) has shown the relevance of experimenter expectancy effects to the topic of mediational factors and expectancies that influence clinical judgments.

Naturalistic, social, and organismic measures.—We have chosen to deal with selected general issues related to personality assessment as well as to summarize a number of studies dealing with some of the traditional assessment devices. Arthur (11) has reviewed a number of recently emphasized alternatives. Two salutary trends are those involving the use of (a) unobtrusive and naturalistic measures, and (b) assessment of behavior in ongoing social situations. For example, a study has been reported in which supporters of presidential aspirant George Wallace were found to violate a particular county law more frequently than did those who were less enthusiastic about him. The assessed characteristic was whether or not cars in public parking lots bore Wallace for President bumper stickers (313). Social behavior is receiving increasing attention as an object of quantitative assessment (15, 178). Bales' book contains a manual for analyzing group fantasy themes, role types, and personality types. He offers a theoretical model derived from factor analysis in which there are three dimensions: 1. group structure, 2. personalities of group members, and 3. relationships among the members. Another approach to assessment that is promising involves the use of organismic and life history data as predictors of behavior. Illustrative of this approach are the use of sex, blood groups, zygosity, and birth order as assessed individual difference variables.

Naturalistic observation can appreciably broaden the horizon of the researcher by showing him that there is more to behavior than he may have originally envisioned (117, 271). Piliavin, Rodin & Piliavin (233) reported a study of helping behavior based on naturalistic observation in the most unlikely of all "laboratories," the New York subway. The growing awareness of the value of naturalistic observation both in defining experimental conditions and in assessing their impact on behavior is a promising development. Barker's (23) ecological approach and a variety of clinical observational procedures have been extended to a large number of behavior settings. Willems & Raush (306) have edited an important volume on naturalistic viewpoints that has implications both for experimental and ecological research. In it Sechrest (262) shows how the effectiveness of experimental methods aimed at changing attitudes can be assessed using ingenious non-reactive and unobtrusive measures. Several articles in the book provide valuable clues to theoretically relevant aspects of behavior often neglected in experimental personality research.

CONSTRUCT VALIDATION APPROACHES

Flowing from Cronbach & Meehl's (78) seminal paper on construct validity has been a diversity of efforts at making assessment data more theoretically meaningful. The most popular of these has involved the incorporation of individual difference variables such as need for achievement, test anxiety, and internalization-externalization within experimental paradigms. The aim of construct validity studies is to extend the boundaries of person-

ality beyond direct prediction to specified criteria. What is wrong with predicting to specified criteria? Obviously nothing. Unfortunately, there are too few of them for those who strive to study personality by relating nonobservable predispositions, attitudes, and tendencies to overt behavior.

The idea of construct validity is not new and continues to exert a powerful influence over efforts to develop theoretically meaningful personality variables. However, like democracy, has it really been tried out? It is seldom, after a scale or test designed to measure a theoretically important dimension has been studied for some time, that the question, "Does this test measure what it purports to measure?" can be unequivocally answered in the affirmative. More often, a bewildering array of inconsistent and unexpected findings in studies employing the scale suggest that whatever is being measured, it isn't what was originally assumed. Different methods of measuring the "same" variable are found to correlate poorly. New scales appear, are the focus of intense interest for a period of time, then drop from sight as negative evidence regarding their construct validity accumulates. The current state of affairs in relation to a number of personality measures is enough to make even the most confident personality psychologists a bit gun-shy.

A case in point is Byrne's Repression-Sensitization (R-S) Scale. Originally advanced as a measure of approach-avoidant tendencies in response to anxiety-provoking stimuli, the scale is now generally regarded as reflecting more complex processes (35, 52, 198, 220, 228). Several findings do seem consistent, however, with the original approach-avoidance construct (257, 289). One possible reason for the inconsistent pattern of results in studies employing the R-S scale is that the scale lacks discriminant validity, i.e. it correlates too highly with measures of constructs which are supposed to be different from R-S. Thus, the R-S scale correlates highly with manifest anxiety even when overlapping items are omitted (283), and with neuroticism (83). Since discriminant validity is lacking, the definition of the construct tapped by the R-S scale is ambiguous.

Some of the puzzling and inconsistent patterns of results obtained with personality measures are, in all likelihood, outgrowths of the questionable implicit assumption that subjects who occupy a similar position on a given personality dimension are (at least as a group) also alike on other variables which may influence the dependent variable behavior. There is no reason on logical grounds to assume that this will be the case in any given instance. Much of the within-group variance in studies comparing groups differing on a given dimension might be reduced substantially by concomitantly taking other personality variables into account. Atkinson and his associates (12) have found that taking test anxiety (fear of failure) into account has (a) increased the predictive precision of achievement motivation studies and (b) contributed to theoretical advances.

Several recent studies have also shown that assessing defensiveness by

means of the Marlowe-Crowne scale meaningfully discriminates between groups of low scores on scales having pathologically toned items, such as the Taylor Manifest Anxiety Scale (MAS) and the R-S Scale. Subjects may receive low scores on such scales either because they are free of the symptoms reflected in the items or because they defensively deny such symptoms. Boor & Schill (38) found significant differences between high and low MAS groups on digit symbol performance *only* when defensive low scorers were eliminated. Schill & Althoff (257) found defensive or "true" repressors on the R-S dimension to have higher auditory thresholds for sexual sentences than did either sensitizers or nondefensive low R-S scorers. They suggest using a measure of defensiveness as a matter of course in R-S studies in order to differentiate the "true" repressor from normals who lack the pathology implied in the test items.

It seems reasonable to assume that personality variables interact with one another in the same manner as do personality and situational variables. Since the form of the interactions quite likely differs across criterion situations, we should not be surprised at the confusing arrays of findings which often occur when an attempt is made to demonstrate construct validity. It may be that attempts to establish the construct validity of a given instrument have a low likelihood of success unless (a) the variable in question is a prepotent one or (b) other personality variables which influence criterion behaviors are controlled or at least taken into account.

Inventory developers have long emphasized the value of developing multitrait rather than uniscale personality measures. It makes sense to study constellations of personality attributes rather than to restrict ourselves to studies of individuals differing on a single dimension. No MMPI researcher seeking to demonstrate a relationship between elevation of the Psychopathic Deviate (Pd) scale and frequency of antisocial behavior would simply select individuals having a peak elevation of the Pd scale regardless of which other scales were also elevated. He would realize that, for example, elevations on Pd and Hysteria predict a very different behavioral pattern than do elevations on, let us say, Pd and Hypomania. Yet, this is essentially what uniscale researchers do when they compose their experimental groups on the basis of scores on only one personality scale. If personality variable interactions are as complex and important as, for example, actuarial MMPI data suggest, perhaps we should be surprised that uniscale investigators obtain predicted results as frequently as they do. The availability of multitrait tests such as Jackson's (149) Personality Research Form and Cattell's 16 PF can stimulate more emphasis on complex personality types. In any event, personality measurement continues to be one of the most critical current problems confronting the student of personality. However, ignoring individual differences because of the problems involved in measuring them seems tantamount to throwing out the baby with the bath water.

In order to demonstrate the construct validity of a test, it is necessary to

show not only that the test has convergent validity (i.e. that it correlates appropriately with theoretically relevant criteria), but also that it has discriminant validity, or independence from theoretically irrelevant criteria. Since Campbell & Fiske (55) originally formulated the convergent-discriminant validity distinction, attempts to demonstrate the convergent and discriminant validity of personality assessment devices have met with only limited success. This seems due not only to test and criterion problems but also to problems connected with conventional correlational and factor analytic methods of assessing such validity. Jackson (150) has developed a new technique called "multimethod factor analysis" for the assessment of convergent and discriminant validity. This method has the advantage of eliminating intruding method variance by analyzing only variance common to more than one method of measurement while eliminating methods variance due to a single method of measurement. The technique appears capable of yielding a larger number of factors corresponding to traits than do conventional factor analytic techniques. It would appear to constitute a valuable new tool for the assessment of construct validity.

The empirical attention commanded by various personality constructs continues to follow the trend of recent years. Anxiety, locus of control, social desirability, introversion-extroversion and repression-sensitization are attracting substantial research efforts.

Anxiety.—Spielberger, Lushene & McAdoo (273) discussed theoretical and methodological issues relevant to the measurement of anxiety. They suggest that much of the confusion in anxiety research results from a failure to distinguish conceptually between anxiety as (a) a stable trait of anxiety proneness (trait anxiety: "How anxious are you generally?") and (b) a transitory emotional state (state anxiety: "How anxious are you now?"). Support for this distinction has been reported in a number of studies. Hodges & Spielberger (143) found state anxiety, but not trait anxiety, to be negatively related to Wechsler Digit Span performance. The hypothesis that trait anxiety measures would be insensitive to conditions of administration differing in degree of stress while scores on state anxiety measures would increase as a function of stress was supported in a study by Allen (5). O'Neil, Spielberger & Hansen (223) reported both state anxiety and blood pressure increases while subjects worked on difficult programmed learning problems and decreases when they responded to easy problems. High state anxiety subjects made more errors on difficult materials and fewer errors on easy materials than did low state anxiety subjects. Gorsuch (114) and Hodges & Felling (142) found support for Spielberger's proposal that trait anxiety scores reflect a predisposition to experience heightened state anxiety in situations involving the possibility of failure or threats to self-esteem, but not in situations involving physical pain or danger. The state-trait distinction may be useful in clarifying some of the inconsistent findings

in anxiety research. Measures of state anxiety obtained in the criterion situation appear to relate more highly to behavior in that situation than do trait measures which might have been obtained weeks earlier.

A topic of recurring interest has been the effects of anxiety on intellectual and cognitive performance. Mazzei & Goulet (204) found an interaction between intelligence and test anxiety in a study on concept formation. Anxiety enhanced the performance of low intelligence college subjects while having a deleterious effect in high intelligence subjects. Morris & Liebert (214) drew "worry" and "emotionality" items from the MAS and studied the effects of these variables on timed and untimed intelligence test performance. Emotionality differences were unrelated to performance. Low-worry subjects who were timed were superior in performance to those who were untimed, while the opposite pattern was found in the case of high-worry subjects, suggesting the possibility of an optimal motivational level. An optimal motivation formulation also gains support from a study by Straughan & Dufort (280), who found relaxation to be associated with better paired-associate performance in high anxiety subjects and poorer performance in subjects low in anxiety. In contrast with the usual finding, Mukherjee (215) found scholastic performance to be *positively* related to MAS scores, but only when ability indices were statistically controlled.

The relationship of anxiety to task performance continues to be the area of greatest concentration in anxiety research. The past year saw anxiety studied in relation to such variables as form discrimination and size estimation (195), stimulus generalization (217), motor learning (60), and verbal learning (62, 177, 270, 288). Shelton & Hill (263) found that achievement anxiety was positively correlated with cheating on tests, but only when the subjects had knowledge of reference group performance.

Studies of anxiety differ widely in terms of the explicitness of their construct validation aims. Some are designed to test theoretical predictions while others are of the "let's see if anxiety is related to variable X" variety. While the former type of study is more likely to be of value in terms of hypothetico-deductive theory construction, the latter has the effect of widening the range of behaviors studied in relation to anxiety. Anxiety has also been studied in a number of interpersonal and clinical situations. Owen & Pedersen (226) found no relationship between MAS scores and accuracy of interpersonal perception, while Compton (70) found IPAT anxiety scores related to body perception in women. Handal (130) reported death anxiety to be negatively related to subjective life expectancy in females but not in males. General anxiety was related to death anxiety in both sexes. Tryon (294) found no relationship between anxiety and thumbsucking in children.

Locus of control.—Generalized expectancies regarding internal vs external control of reinforcement have been related to a variety of behaviors. Williams & Vantress (308) found a low positive relationship between Rotter's Internal-External (I-E) scale and self-report hostility and aggression.

They attribute the relationship to the frustration attendant to the externals' perception that they have little control over their destinies. Several cross-cultural comparisons of I-E scores have been reported. In one of these (147), Chinese-American students were found to be significantly more external than Anglo-Americans and significantly more internal than Hong Kong Chinese students. Danish and American students were compared on locus of control by Parsons, Schneider & Hansen (229). While the two national groups did not differ on I-E, they did differ in attribution of I-E attitudes to other groups. Danes ascribed greater internality to American and West German students, while US students attributed greater externality to West German, Japanese, and other American students. These results have implications for the analysis of components of national stereotypes.

Experiments have linked locus of control to such variables as verbal conditioning (24), interpersonal evaluations (158), the prediction of unethical behavior (243), perception of moral sanctions (2), and attention deployment in experimental situations (175). The latter study is of particular interest with regard to the construct validity of the I-E scale. Two experiments demonstrated attentional differences between internal and external subjects, with internals appearing more vigilant and observant. The group differences increased as a function of degree of uncertainty introduced into the experimental situation and decreased when instructional cues made it clear that attention was the focus of the experimenter's interest. The results point to more active commerce with the environment on the part of internals.

The I-E dimension has been studied in relation to clinical phenomena. Abramowitz (1) found I-E scores related to self-reported depression in college students. Harrow & Ferrante (134) studied locus of control in psychiatric patients and found that on intake, schizophrenics and depressives scored in the external direction, while manics were markedly internal. After 7 weeks of hospitalization, the depressives showed a significant shift in the direction of greater internality, while the manics showed a slight shift toward the external end of the dimension. Smith (268) studied the I-E scale in relation to life crisis resolution. Patients undergoing crisis intervention treatment became significantly more internal over the 6-week crisis resolution period, while a control group of noncrisis patients undergoing conventional psychotherapy showed no change in I-E score. Williams & Nickels (307) found that externally oriented subjects scored higher on indices of suicide potential than did internally oriented subjects. While this finding meshed nicely with theoretical expectations, it was also found that scores on a measure of accident proneness were positively correlated with scores on both the externality and suicide potentiality scales.

The I-E scale was designed to measure generalized expectancies regarding locus of control. Recent studies suggest, however, that the assumption of unidimensionality is not tenable (209, 258). Several dimensions of control are apparently being tapped by the scale. Future research might very profitably concentrate on the development of situation-specific I-E measures

(e.g. social, academic) or ipsative measures of locus of control in various types of situations. Predictive utility of the I-E dimension would very likely be enhanced by proceeding in this direction.

Social desirability.—Social desirability researchers are taking one of two tacks. One group of studies deals with social desirability as a response set and the factors which influence social desirability ratings. A second emphasis is on social desirability as a personality variable (e.g. need for social approval or defensiveness) that is meaningfully related to other behaviors.

Carr (59) confirmed that instructional set can influence the social desirability values ascribed to personality test items. Age differences in peer ratings of socially desirable and undesirable behavior were noted by Miltorn & Lewis (208), while Crandall & Gozali (75) found Children's Social Desirability Scale scores to be higher among both American and Norwegian children of high religiosity than among less religious children. Gynther, Gray & Strauss (127) also found religious persuasion to affect social desirability ratings of MMPI items having religious content.

The popularity of a purely psychometric approach to the need for approval (or defensiveness) variable has waned somewhat, but its use as an individual difference variable has increased. In a mixed-motive game situation, Tedeschi, Burrill & Gahagan (286) found that high need for approval subjects tended to be submissive to power and penitent following defection. Lefcourt (174) successfully predicted increased defensiveness on a projective test in high need for approval subjects when expectancies of possible disapproval by the experimenter were aroused. Lefcourt also found evidence for the defensive-nondefensive repressor distinction referred to above in our discussion of the R-S scale.

Self-reinforcement, or self-approval, which has seldom been considered in previous approval motivation research, may be a factor of great significance in the maintenance of behaviors which are considered by the individual to be socially desirable. In a study by Matell & Smith (203) college students were administered two course examinations under conditions differing in the degree to which performance was relevant to academic achievement goals. Under high-relevance conditions, need for approval was unrelated to test performance. Under low-relevance conditions, the approval motive was highly related to performance in high and moderate ability groups, but not in a low ability group. In view of the absence of extrinsic approval incentives in the latter condition, the results seemed to fit with an hypothesis that the approval motive cues off classes of behavior which have been reinforced by the approval (or avoidance of disapproval) of others in the past, but which are currently self-reinforced.

BIRTH ORDER

During the past decade there has been a somewhat uncontrolled surge of interest in the relationship between behavior, both normal and abnormal,

and birth order. The history of birth order research has been marked by controversy regarding not only the methods used in determining whether the observed distribution of birth ranks in a sample differs from the expected distribution, but also regarding sources of bias which may distort the observed distribution of persons by ordinal position. Price & Hare (237) offer a thoughtful analysis of such sources of bias. Although their data are based on English and Welsh census data, the methodological problems which they discuss do not appear to be limited to specific geographical locales.

Schachter's (256) work on birth order and affiliative behavior under stress continues to stimulate research. Several recent studies have investigated the relationship between ordinal position and affiliation in nonlaboratory settings. Characteristically there have been conflicting results in inquiries concerning ordinal position. Baker & O'Brien (13) found, contrary to their hypothesis, that later-born rather than first-born college males showed a higher relative frequency of joining fraternities during their freshman year in college. They suggest that first-borns, accustomed to a superior position in the family, may be reluctant to adopt the subordinate role of a fraternity pledge. Murdoch & Smith (219), on the other hand, found a higher relative frequency of fraternity membership among first-borns than among later-borns. There is growing evidence that the interactions between sex, birth order, and various affiliative indices are complex.

One reason for the inconsistent pattern of results and difficulties in replication when birth order is employed as an independent variable undoubtedly is due to the rather common gross breakdowns of subject samples into first-borns (including only children) and later-borns. Often totally ignored are such factors as family size, sex (236), age of parents, sibling sex, and spacing of children in the family. These variables may well affect the early psychological climate of the family and interpersonal relationships experienced by the child as much as or more than birth order alone. For example, Jamieson (153) found that birth order was unrelated to risk-taking behavior, while family size was significantly related (children from smaller families, regardless of their ordinal position, accepted less risk than children from larger families). A more highly dimensionalized breakdown of subject samples in future research, taking into account factors such as those mentioned above, might result in more consistent findings. Too often, because a variable is found to be unrelated to one class of behaviors, it is assumed to be unimportant when studying other dependent variables. A case in point is family size, which was found to be unrelated to birth order and affiliative behavior under stress in Schachter's (256) early studies.

A study of affiliative behavior under stress by Robertson & Dotson (244) suggests that ordinal position may interact with both psychophysiological and parental variables in a complex manner. A higher frequency of affiliative behaviors was found among first-born subjects who reacted to achievement stress with an epinephrine-like stress response and who per-

ceived their parents as affectionate and indulgent; when the physiological stress response was norepinephrine-like among first-borns, increased parental affection and indulgence was associated with decreased affiliation.

Birth order studies have dealt with an almost overwhelming variety of behaviors. MacDonald (193) found first-born subjects to exhibit a higher degree of religious affiliation with more frequent church attendance, particularly in the case of females. Bradley & Sanborn (40) reported a significant over-representation of first-borns in a group of over 1500 children chosen over a 10-year period by teachers and counselors for membership in a special program for superior students. Eisenman & Robinson (96) reported a study showing first-born males to be significantly more creative than later-borns on three separate measures of creativity. No significant differences were found between first- and later-born females on any of the creativity measures. One study clearly illustrated the importance of simultaneously examining personality, birth order, and situational variables in affecting behavior (252). It revealed no relationship between birth order in males and serial learning under neutral and achievement-oriented conditions. However, when test anxiety was also taken into account, a clear pattern emerged, with high test-anxious first-borns performing more poorly under achievement-oriented conditions than any of the other groups.

Evidence that (a) first-borns are more highly socialized and more conforming to societal norms (e.g. of the strong male and the dependent female) and to expectations of authority figures, and that (b) this affects their behavior in experiments is reported by MacDonald (194). A significantly higher proportion of later-borns failed to appear for their appointments. Under threat of painful electric shock in the experimental situation, first-born males were least likely, later-borns more likely, and first-born females most likely to express the wish to withdraw from the experiment before being actually subjected to shock. First-borns were also less likely to reveal suspicions that the experiment was to be different than the experimenter represented it.

Birth order when ignored or too grossly assessed would appear to be a source of potential confusion in personality research. Previous research (56) has shown that first-borns are more likely than later-borns to volunteer for experiments. This, together with the higher probability that later-borns will fail to appear for the session, raises the possibility that first-borns may be over-represented in many samples. On a theoretical level, the MacDonald (194) paper makes a notable contribution by introducing a conceptual framework within which to consider birth order research. Birth order is the sort of variable that some experimentalists find annoying. Their psychological interests lie elsewhere (dissonance, anxiety, locus of control) and they resist having to attend to it. This resistance is strengthened by reports in the literature of conflicting results. What the conflicting results suggest quite clearly is that the interactions of birth order with other vari-

ables (including manipulated ones) are poorly understood. Under such circumstances, ignoring variables in which we are not immediately interested can be hazardous indeed.

SEX

Perhaps the most frequently differentiating organismic variable is sex. Our comments concerning birth order are probably even more applicable to the sex variable. It is somewhat surprising, therefore, that this intuitively and, as we shall show, empirically important variable is frequently ignored in personality research. The extent to which this is the case is indicated by a survey of the 1968 publications in two leading personality journals, the *Journal of Personality and Social Psychology* and the *Journal of Personality*, undertaken by Carlson (58). Less than half of the studies which could have tested for sex differences did so, but among those which did, 74% found significant sex differences.

Our review of the 1969 literature disclosed no reversal of this trend toward ignoring an obviously relevant facet of personality. Many researchers either employ subjects of only one sex, or describe their samples as, for example, "90 introductory psychology students." Many widely accepted, "established" findings in personality research are based on single-sex subject samples. Unfortunately, in view of the fact that few studies are ever replicated, the probability that important sex differences regarding such phenomena will be revealed in future research seems small. This is compounded by the tendency in some research areas to study one sex almost exclusively. A prime example of this latter trend exists in research on achievement motivation. Because of studies in the early 1950s by McClelland and his associates which suggested that achievement motivation in women reflected the reinforcement value of social approval rather than of achievement, later research has utilized males almost exclusively. In view of evolving changes in cultural conceptions of female roles and aspirations, greater contemporary research emphasis on achievement motivation in women seems warranted.

In earlier sections of this review, sex differences were noted with regard to a wide variety of behaviors. Sex differences have also been found in such diverse areas as mixed-motive game behavior (30, 113, 145, 200, 287), in-group-outgroup evaluation (238), attribution of success or failure to personal or external factors (103), and modeling effects (315). Montanelli & Hill (212) found male experimenters to elicit higher achievement expectancy statements and base rate performance from their subjects than did females. Female subjects were reported to show less vicarious emotional arousal than males (73). Many other studies showing sex differences could be cited at the cost of belaboring the point that sex differences are important and should be systematically explored in personality research.

PERSONALITY DEVELOPMENT

Research in personality development consists of admixtures of three major emphases. One of these comprises fact-finding studies of developmental trends. These contrast with our emphasis on individual differences in that they are basically concerned with developmental commonalities in behavior. A second type of study is concerned with antecedents of individual differences in personality. Finally, there is the emphasis on the process of behavior acquisition, differing from studies of developmental trends in that it is basically concerned with experientially mediated changes in behavior rather than those attributable primarily to maturation. An example of this type is current research on observational learning, or modeling.

DEVELOPMENTAL TRENDS

A number of studies in 1969 investigated developmental trends through cross-sectional designs (160, 277, 301). For example, Druker & Hagen (92) studied selective information processing as a function of age. Many social learning situations require the individual to attend to certain relevant stimuli while ignoring irrelevant cues. The generally poorer performance of younger children on learning and discrimination tasks is frequently attributed to their difficulty in selectively attending to relevant cues. In a comparison of fourth, sixth, and eighth-grade children, Druker & Hagen found that more efficient encoding and attentional processes, rather than improved visual discrimination, were responsible for the greater efficiency of the older subjects in processing task-relevant and task irrelevant information.

A notable longitudinal study of the development of the body concept is one described by Fatereson & Witkin (102). Using human figure drawings to define differentiation, as reflected in articulation of body concept, it was found that degree of articulation increased between ages 8 and 14, after which it remained remarkably stable over as long as a 14-year span. These findings complement the results of Witkin's earlier field dependence-independence work in the area of perception.

A growing number of developmental studies deal with the child's relationship to the world beyond his family and school. McClintock & Nuttin (205) studied competitive game behavior in American and Belgian children and found that older children make more competitive responses in both cultures, but that until the sixth-grade level American children tend to be more competitive than Belgian children of the same age. In a study of political attitudes in young children, Rebelsky, Conover & Chafetz (239) interviewed children between the ages of 2 and 13. As expected, amount of political information increased with age. But it also turned out that children under age 6 do not have the cognitive maturity to meaningfully accumulate political information. In contrast to Hess & Torney's (138) contention that the child initially relates to the political system through the President, Rebelsky et al found that

children knew more about controversial contemporary political issues than they did about the President (including his name). Such findings illustrate the danger of generalizing over time about the development of political and societal concepts.

Moving from the society at large to one of its major components, the schools, evidence has been presented indicating that girls tend to focus on negative aspects of academic situations more than do boys. As a consequence, they are more prone to be high in test anxiety. Boys' expectancies about their future achievement tend to exceed past feedback, while girls' estimates fall below their level of past reinforcement (74). Could such a result be attributable to differences in achievement training? While this may be the case, the situation is unclear because of the complexity of concepts such as need for achievement. For example, we tend to think of achievement training as being intimately tied up with independence training. Yet empirical evidence has been gathered indicating that mothers' values about each of these types of training are orthogonal to each other. Increased knowledge of children's anxieties, strivings toward independence, and achievement orientation is of direct relevance to important social problems such as desegregation and the entire educational enterprise (267).

· RESPONSE TO REINFORCEMENTS

An example of developmental research in which personality variables are very likely implicated is that of response to reinforcement. The effects and meaning of reinforcements have received considerable attention in relation to social development. Hill & Morley (141) studied differences in responsiveness to social reinforcement between younger (first and second-grade) and older (fourth and fifth-grade) children as a function of sex and instructional conditions. Among the younger children, response rate on a marble-dropping task increased with social reinforcement and decreased under nonreinforcement conditions regardless of whether the task was represented as a game or as a test. A very different pattern emerged for the older subjects: for males, performance did not differ in the "game" condition as a function of reinforcement condition, while, surprisingly, in the "test" condition, the response rate decreased in the social reinforcement condition while increasing in the nonreinforced condition. In contrast, older females showed greater responsiveness to positive social reinforcement under test conditions. In addition to indicating that younger children are less responsive to instructional orientations, the surprising pattern of results among older males suggests that the functional properties of social reinforcement may interact in a complex manner with other variables. In one comparison of the potency of three variables, it was found that social reinforcement exerted more influence on the performance of children than did either sex of subject or sex of experimenter (212). On a more general level, individual differences in responsiveness to various classes of reinforcing stimuli may re-

flect underlying variables which are of special interest to personality researchers (181). Masters (202) has demonstrated that social comparison processes may affect the value of a reinforcer.

There is growing evidence of large sex differences in a variety of areas, such as achievement attitudes, and intellectual performance. An important problem that can be answered by longitudinal inquiry concerns the stability of these differences. When do they become evident? Do the differences dissipate over the years? With regard to the effects of social reinforcement, feedback's facilitating effects have been found to be greater for boys than for girls at ages 8 and 9 and also for 13-year-olds (140). Sex differences in social behavior have also received attention, and in one analysis ordinal position was related to 64 types of family interactions involving children and their parents (293).

BEHAVIORAL ANTECEDENTS

There is growing interest in that aspect of developmental personality research concerned with antecedents of individual differences. Studies in this area generally address themselves to the role of parental or familial antecedents of behavior.

Wohlford (310) tested the hypothesis, derived from identification theory, that cigarette smoking in young adults is related to parental smoking behavior. The degree of commonality of smoking habits between college males and their fathers was highly significant in intact families, while in broken families there tended to be an inverse relationship between father and son smoking behaviors. No clear relationships were found between father-daughter, mother-son, or mother-daughter smoking patterns.

Vogel et al (298) studied sex-role stereotypes in college students as a function of maternal employment. They found that both male and female children of mothers who had worked outside of the home at some time during the children's lives perceived less difference between the male and female roles than did children of unemployed mothers. Women whose mothers had worked perceived the masculine role as significantly less masculine, and the female role as significantly less feminine than did those whose mothers had not worked. Men whose mothers had worked were found to perceive the masculine role as significantly less masculine. Daughters of working mothers saw the feminine role as entailing more competence; sons of working mothers tended to perceive the feminine role as involving less warmth and expressiveness than did sons of nonworking women. No differences in self-concept were found between the children of working and nonworking mothers. Vogel et al see as the most important implication of their results the possibility that experiential variables which decrease the difference between male and female sex-role stereotypes may serve to contribute to a greater behavioral sex role equality in our culture.

Level of aspiration.—In our achievement-oriented society, level of aspira-

tion is a very salient variable. In spite of its importance, however, there are few systematic studies of its child-rearing antecedents. Crowne et al (79) followed up a sample of 83 young adults whose mothers had reported their child-rearing practices in the Sears, Maccoby & Levin (261) studies. The subjects performed on the Rotter Level of Aspiration (LOA) Board and their LOA patterns were correlated with their mothers' scores on the Sears et al rating scales. Although a very large number of relationships were studied, one stable and significant pattern emerged. Defensively high goal setting was found to be associated with early parental punitiveness toward aggression and dependency, high maternal anxiety, and a higher frequency of obedience problems. Goal setting characterized by a high number of shifts, suggesting unstable goal attainment expectancies, was associated with mothers who made frequent use of tangible rewards and who were rated as being less warm while also less likely to be rejecting.

The contemporary social relevance of the level of aspiration variable is suggested in a study by Guggenheim (124), who studied the relationships between self-esteem, achievement expectations, and discrepancies between achievement expectations and actual achievement in sixth-grade Negro and white students in an urban school setting. Negro children were found to have significantly greater discrepancies between achievement expectations and performance than white children, with their expectations tending to far exceed their actual performance. Guggenheim suggests that the unrealistic level of aspiration in Negro children constitutes a defense against a cultural milieu which constantly undermines their sense of integrity and worth.

If this be the case, the question of how the child then deals with the inevitable frustration arising from his inability to meet his aspirations becomes a question worthy of further research. Undoubtedly, some children react by rejecting academic aspirations. The child-rearing patterns of parents of less well-educated background, and likely of less ability, as well as the realistic life conditions of the child growing up in a low income family setting are such as to inhibit the development of a sense of competency and high aspirations, and the acceptance of outcome (44).

Parental and child-rearing antecedents of creativity (87), self-disclosure (90), deviant childhood and adolescent behaviors (8, 218), academic underachievement (116), and homosexuality (100, 125) have also been explored. The positive results obtained in discriminating between groups of parents and families in relation to an amazingly wide variety of dependent variable behaviors in children underlines the crucial and complex role of familial antecedents in the development of behavior.

BEHAVIOR ACQUISITION

The past 5 years have witnessed the ascendance of research on social learning processes (109, 227). Inspired by the provocative work of Bandura and others, a veritable torrent of studies concerned with vicarious learning processes has flowed from the laboratories of developmental and personality

researchers. A social learning boom in clinical domains persists and bears fruit (16).

Modeling.—Studies of modeling illustrate the social learning approach as applied to personality development. They fall into two broad categories. The first one consists of experiments mainly concerned with establishing the range of influence of modeling procedures. These studies ask the question, "Can behavior X be established or modified through modeling procedures?" The second type of study is more concerned with interpersonal processes. The focus here is on relationships among the model, the observer, and the situation. These variables combine in influencing the outcomes of observational learning. While both areas provide useful information on an important social learning determinant of individuality, the second provides especially rich opportunities to study the effects of extant individual difference variables on this social process.

Several studies have investigated the relationship of modeling influences to moral judgment. Cowan and co-workers (72) performed a study designed to replicate and extend an earlier one by Bandura & McDonald (18) which showed that children's moral judgments could be modified through exposure to adult models. Children giving high and low level moral judgments to Piaget-type moral judgment stories were exposed to an adult model who responded at the opposite level. Modeling emerged as a significant determinant of later moral judgments regardless of the direction in which the behavior was being modified. It was also found, however, that other variables, singly or in combination (such as pretest scores of the subjects, measure of moral response, amount of time between conditioning and post-test, and type of item) served to influence the degree to which later moral judgments were affected by exposure to models. All in all, however, the results constitute an impressive replication of the earlier study. Cowan et al raise a number of theoretical issues regarding the extent to which the demonstration of modeling effects is relevant to Piaget's stage formulation and the degree to which social learning theory is able to account for the development of moral judgment. In a reply to the paper by Cowan et al, Bandura (17) clarifies the social learning position on moral development and suggests the possibility that modeling influences are more important in moral development than stage theories such as Piaget's would lead us to believe.

Kohlberg (166) carried out a series of studies on moral development strongly influenced by Piaget's developmental ideas. Kohlberg believes that the child passes through successive stages of moral judgment. The rates at which these stages are achieved are influenced by experience. Thus, the child's identification with his parents can speed up or slow down the cognitive-developmental progression toward maturity, and the child's stages of development, in turn, influence the nature of his relationships within the family. Kohlberg concludes that there is no generalized "identification-in-

ternalization" factor in children's personalities and that the presence of a same-sex parent is not necessary for normal development. In addition to the question of whether or not all children go through an orderly sequence of stages of moral thought, there are a number of methodological issues to be settled. For example, experiments based on Kohlberg's cognitive-developmental theory have not definitively explicated the interaction between moral and cognitive development (176, 242, 260). There are mensurational questions as well. How is moral level measured? Can it be scored reliably?

In an investigation of moral standards, from a social learning standpoint, Allen & Liebert (6) studied the role of live and symbolic deviant-modeling cues on adoption of a previously learned self-reinforcement standard. Within an information value paradigm, it was predicted that live modeling cues would exert a greater effect on subsequent behavior than would symbolic cues (statements by the model regarding the self-reinforcement standard which he had employed). The results of two experiments supported the hypothesis and also indicated that the two types of modeling cues produced an additive effect.

In two studies, Thelen (290) and Herbert, Gelfand & Hartmann (136) investigated the effects of exposure to self-critical models on subsequent self-critical behavior in observers. In the former study, significant modeling effects were found in the number of self-blame and rationalization statements made by subjects during subsequent failure trials on an experimental task. The latter study found modeling effects on the adoption of the self-critical model's self-punitive standards, but not on self-critical verbalizations. Observers' self-esteem was unrelated to adoption of self-critical behaviors. A number of studies have demonstrated that modeling can influence the syntactical language productions of both normal (188, 291) and retarded (224) children. Fryrear & Thelen (106) found significant modeling of affectionate behavior in nursery school children.

Several nonlaboratory investigations dealt with the role of same-sexed models in personality development. Bell (26) studied the effects of father role modeling on sons' vocational behaviors. While fathers served as the most important role models for their sons in adolescence, nonparents became more significant as sons became young adults. Sons having the most positive paternal role models during adolescence were found to function most effectively as young adults. Wohlford et al (311) found that preschool father-absent Negro males were more feminine, less aggressive, and more dependent than father-present counterparts. No differences were found between father-absent and father-present females. Older sibling structure in father-absent families was found to influence such traits as aggression, masculinity-femininity, and dependency.

While most laboratory studies of modeling have been primarily concerned with investigating behavioral changes attributable to modeling effects, some studies have investigated the role of individual difference, situational, and process variables in modeling. Allen & Liebert (7) found that

providing incentives for nonimitation of a model's self-reward standard reduced modeling effects, while the amount of experience on the task attributed to the model positively affected modeling of self-reinforcement at a performance level approximating that of the model's standard, but not at nonapproximating performance levels. A study by Liebert & Fernandez (186) indicates that task complexity is inversely related to imitative learning and that vicarious reinforcement enhances such learning only under high complexity conditions. They suggest that vicarious reward serves primarily to focus the observer's attention on the relevant modeling cues. Liebert et al (187) also studied the effects of describing a model as "friendless" on imitation and prosocial behavior toward the model. While no differences in imitative behavior were found between "friendless" and neutral model conditions, more prosocial behavior, in the form of donating tokens, was directed toward the "friendless" model. In another study of model differences, Fryrear & Thelen (106) found that females modeled affectionate behavior to a significantly greater extent when the model was a female than when he was a male. Sex of model did not differentially affect modeling effects in male children.

Sorely needed process studies designed to identify the mechanisms whereby modeling effects are mediated have begun to appear. Liebert & Fernandez (186) tested hypotheses relating to the informational role of vicarious reinforcement. Horowitz (146) found that focusing on the ability of the model significantly affected modeling behavior, but only in the case of a low-competence model. Van Hekken (297) focused on the role of verbal mediational processes in vicarious learning. Children who were instructed to verbalize the model's behavior showed greater imitation than did children under irrelevant or nonverbalization conditions. However, this was the case *only* for subjects who were classified as mediators on the basis of a reversal shift pretest.

In an incisive review of the socialization process, Zigler & Child (317) observed that a recurring issue has been whether the child is regarded as an active or passive agent. Is the child at the mercy of an invariant maturational sequence? Is his behavior shaped primarily by environmental contingencies and observational opportunities? Socialization is not simply a matter of controlling impulses and acquiring norms and internalizing a parental superego. It is closely correlated with changing biological events. As the child develops he definitely becomes more active than passive, *vis-a-vis* his environment. For the student of personality, assessment of the passive-active balance at different development points would seem to be very worthwhile endeavor.

PERSONALITY AND SOCIAL BEHAVIOR

The line between the psychologies of personality and social behavior continues to be vague. In this case the vagueness is salutary. While defini-

tions of personality are many and varied, there is general agreement that it finds perhaps its fullest and most meaningful expression within the social arena. The work of the social psychologist is then of special relevance to the personality researcher. One would hope that the social psychologist would be interested in the major areas of personality research. Regrettably, as one surveys the social psychological landscape, a glaring feature is the relative absence of references to individual difference variables. This is unfortunate, since many of the phenomena studied by social psychologists would seem to offer unique opportunities for understanding the interactions between personality and social-situational variables. Such interactions could serve to modify inferences drawn from and issues surrounding studies of social behavior.

Several years ago, Kogan & Wallach (165) convincingly demonstrated the manner in which individual differences in personality may serve as "moderator variables" influencing the social psychological realm of risk-taking behavior. It seems fair to assume that similar findings would result in other social psychological areas if individual difference variables were more frequently taken into account. In selectively reviewing several topics of contemporary interest in social psychology, we shall endeavor to highlight empirical questions regarding several personality variables that seem to be especially promising.

ALTRUISTIC BEHAVIOR

Considerable attention is currently being directed toward the study of altruistic or helping behavior. Krebs (167), in an extensive review of this literature, summarized variables shown to influence the occurrence of altruistic behavior. Positive and negative affective states and exposure to models serve to influence the behavior of benefactors, while dependency and interpersonal attractiveness influence the eliciting capacity of recipients. Sex, age, ordinal position, and social class have also been shown to significantly affect altruism. However, attempts to define and measure altruism as a general personality trait have been largely unsuccessful. Bryan & London (45) have comprehensively reviewed the literature on altruism in children.

An important series of studies by Latané and his associates (171, 172) has explored variables influencing intervention behavior during emergencies. These studies, most of which have been conducted in laboratory settings, have consistently shown that subjects are more likely to intervene in an emergency if they are alone than if they are with another person or in a group. Inhibition of helping behavior is especially strong in the presence of a nonreactive fellow bystander. This suggests the importance of modeling effects in influencing persons' interpretations of a given situation and their reactions to it. The fact that in the laboratory the probability of intervention by a group member decreases as a function of group size suggests that diffusion of responsibility also plays a role in bystander intervention. How-

ever, a study by Piliavin, Rodin & Piliavin (233) failed to find a relationship between number of bystanders and frequency of intervention in a naturalistic setting (the New York subway). Another factor affecting helping behavior, cost of intervening, has been studied in relation to other variables. Wagner & Wheeler (299) studied cost of helping (contributing to charity) in relation to need for help and exposure to a generous, neutral, or selfish model. Helping behavior was positively affected by exposure to a generous model and negatively affected by a selfish model under low cost conditions. Under high cost conditions, model effects were not significant. Need for help only weakly affected helping.

The other side of the cost picture was investigated by Epstein & Hornstein (98). Their investigation of the effects of anticipated punishment for *not* helping was inspired by popular suggestions that, under certain circumstances, individuals who fail to help in an emergency should be prosecuted. Neither attraction toward the victim nor anticipated punishment for not helping the victim affected helping behavior. An interaction was found between attraction and cost of not helping. Subjects who liked the victim chose to help more often when punishment was anticipated than when no threat of punishment existed; the opposite pattern obtained in the case of a disliked victim. Greenglass (118) found that subjects who had received prior help from a peer subsequently gave a high degree of help to a second peer regardless of the degree of similarity between the two peers. This suggests adoption by the subject of a norm of social responsibility. Prior hindrance by a first peer, however, resulted in hindrance toward a similar second peer and help toward a dissimilar second peer. Hindrance toward the similar second peer was interpreted as being the result of a negative reciprocity norm or of generalization of resentment toward the hindering first peer, while help given to a dissimilar second peer following hindrance was attributed to the subject's exaggerated awareness of the social responsibility norm.

The importance of the degree to which a person perceives himself as having a responsibility to intervene was demonstrated by Tilker (292) in a Milgram-type obedience situation. Subjects who were made to feel responsible for the well-being of another subject supposedly being shocked in a learning experiment, and who received maximal feedback regarding the discomfort of the learner, were most likely to help the victim by interfering in some way with the experiment. Staub (278) found assigned responsibility to increase the probability of helping behavior in kindergarten and first-grade children. A study of the possible role of empathic responses in mediating helping behavior was reported by Aderman & Berkowitz (4). Subjects who imagined themselves in either the role of an unaided person in need or of a thanked helper gave the most help, while those who attended to a potential helper who failed to provide help were least helpful. Analysis of mood data suggested that helping behavior had been mediated by the plea-

surable empathic experiences of subjects' attending to the thanked helper, and by the unpleasant empathic experiences of those who had attended to the unaided person in need.

It is puzzling that researchers concerned with the types of situations just described have largely ignored the role which personality variables may play in influencing the occurrence of altruistic behavior. In our judgment, variables such as need for social approval, locus of control, and repression-sensitization might well function as moderators of response to them. Investigations of them probably would serve to enhance understanding of the factors involved in helping behavior (3, 157). Would subjects having an internal locus of control be more likely to intervene in an emergency than external subjects? Would need for approval increase the inhibiting effects on helping behavior of other nonreactive bystanders? Given an ambiguous situation which may or may not be an emergency, would repressors, because of their presumed tendency to deny or avoid anxiety provoking stimuli, be less likely to label the situation as an emergency and intervene? These are only a few of the many empirical questions which might be asked regarding the role of individual difference personality variables in helping behavior.

INTERPERSONAL ATTRACTION

The study of interpersonal attraction has received considerable recent attention (82, 241, 245, 274). Much of the work has been performed within the context of Byrne's (49) reinforcement model of attraction, according to which the evaluative meaning of any stimulus is assumed to be a positive linear function of the proportion of positive reinforcements associated with it. In the model, positive and negative reinforcements act as unconditioned stimuli which evoke implicit affective responses, and any discriminable stimulus associated with reinforcement also acquires the capacity to evoke the affective responses which, in turn, mediate evaluative and attraction responses. Byrne (49) cites several studies showing that agreeing and disagreeing attitude statements have positive and negative reinforcement properties, respectively. Consistent with the model, many studies have shown attraction toward another to be a positive linear function of proportion of shared attitudes. Byrne et al (53) have shown that the specific attitude similarity-attraction function derived from college samples also obtains in other nonstudent samples. Only a schizophrenic sample was found to have a significantly different (flatter) function.

Response to political candidates (50) and to black strangers by whites (51) has also been shown to be a function of attitude similarity. In the latter study, attraction toward the black stranger was also significantly affected by the positiveness of the stranger's evaluation of the subject and the subject's level of racial prejudice. The manner in which commonality and desirability of a shared trait interact to influence attraction was investigated by Jellison & Zeisset (155). When a desirable trait is shared, attraction is

greater when that trait is uncommon within the population; when an undesirable trait is shared, attraction is greater if the trait is a common one. Byrne & Griffitt (52) have shown that while awareness of personality similarity contributes to the similarity-attraction relationship, it is not a necessary component of the relationship. Griffitt (121) provided evidence that attraction toward a stranger as a function of attitude similarity is mediated by anticipated positiveness of interaction with the stranger. Stapert & Clore (275) found attraction toward others with agreeing attitudes to be a negatively accelerated function of number of exposures to prior disagreement. In support of the reinforcement model of evaluative responses, Griffitt & Guay (122) showed that evaluation of both human and nonhuman stimuli is a direct function of the proportion of positive reinforcements associated with them. Lott & Lott (191) demonstrated that pictures of liked and disliked persons can function as positive and negative reinforcing stimuli in a learning situation. In a study by Lott and co-workers (190), children expressed greater attraction toward an adult associated with immediate reward, and less attraction toward one associated with delayed reward, than was the case when the adults were associated with an equal number of immediate and delayed rewards. The negative effects of delayed reward on attraction were attributed to frustration effects. Vicarious reward has also been shown to influence attraction toward persons associated with the reinforcing situation (192). In an interesting study of the effects of discrepancies between emitted liking and perceived liking by another, Blumberg (36) found that subjects preferred not only to be liked by those they liked, but also to be disliked by those whom they disliked.

Relatively few of this year's studies explored the role of personality variables in interpersonal attraction, and those which did explore it did not yield especially promising results. Consistent with the results of prior research, Gormley & Clore (113) found no relationship between dogmatism and attraction as a function of attitude similarity and concluded that scores on the Dogmatism Scale do not reliably affect the similarity-attraction relationship. In a study by Byrne & Griffitt (52), scores on the Repression-Sensitization Scale did not influence the similarity-attraction relationship, although the variable affected awareness of similarity, with sensitizers showing higher perceived similarity, particularly in the case of dissimilar strangers. Further research is clearly required to determine the manner in which individual differences in personality interact with other variables in influencing interpersonal attraction. In light of Blumberg's findings, for example, it would be interesting to determine whether need for social approval or social-evaluative anxiety (Watson & Friend 302) would affect the extent to which subjects prefer to be disliked by those they dislike. Would such variables also influence the basic similarity-attraction relationship?

On a methodological level, studies by Baron & Kepner (25) and by Smith, Meadow & Sisk (269) indicate that varying attitude similarity can

serve as an economical method for manipulating attraction, which in turn may be studied in relation to other variables.

AGGRESSION

One way of characterizing research on aggression is in terms of the effects of three classes of variables: 1. those resulting in increases in aggressive behavior; 2. those serving to inhibit aggression; and 3. individual difference variables as they affect aggressive behavior.

In this year's research, variables shown to increase level of aggression include exposure to an aggressive model, level of arousal, and mediating verbal labels. Several studies have again demonstrated the powerful effects of modeled aggression in conjunction with other factors. Hartmann (135) studied aggression in adolescent delinquents as a function of level of anger arousal and exposure to fight films focusing either on the pain being experienced by the victim or on the instrumental aggression. Regardless of arousal level, subjects who observed the modeled aggression behaved more aggressively than did subjects who viewed a nonaggressive control film. Aroused viewers exhibited more aggression than did nonaroused subjects, and exposure to the pain cues resulted in greater subsequent aggression than did exposure to the instrumental aggression film. Individual differences were also found to be important; frequency of past antisocial behavior was positively related to aggression, particularly under conditions of anger arousal and exposure to the pain cues film. Baron & Kepner (25) found that, relative to a no-model control condition, both aggressive and nonaggressive models affected level of subsequent aggression toward a provocateur. The interaction between attraction toward the model and the model's (aggressive or nonaggressive) behavior was not significant. Proportion of aggressive responses made by a model to an anger instigator was not found to influence aggressive behavior by the observer (185). Hanratty and associates (131) found aggression to be increased in children by exposure to an aggressive model even in the absence of frustration or other provocation. Although the children had observed aggression toward a human target, subsequent aggression was stronger when it was expressed toward an inanimate target which was similar in appearance.

In addition to exposure to models, level of general arousal and verbal mediational processes have also been shown to influence aggression. Geen & O'Neal (108) exposed subjects to aggressive or nonaggressive film content before they were asked to deliver shock to an experimental confederate. During this period of time, half of the subjects were stimulated with white noise. Aggression was highest in those subjects who saw the aggressive film and were subjected to white noise. The results are interpreted as supporting Berkowitz's suggestion that general arousal serves to increase the probability of responding with aggressive responses to aggressive cues. The manner in which verbal labels may mediate aggression was suggested in a study by

Berkowitz & Knurek (31). Using classical conditioning procedures, subjects were trained to have a negative attitude toward a certain first name. Greater aggression subsequent to being angered by the experimenter was shown toward a "fellow subject" having the critical name than toward one having a different name, suggesting that conditioned attitudes toward a person's name may serve to mediate aggression in angered or frustrated individuals.

During the past year, variables shown to have inhibiting effects on aggression include humor, self-punitive behavior, and aggression anxiety. Evidence for humor-mediated catharsis was provided by Landy & Mettee (170). Subjects exposed to hostile and nonhostile cartoons after being aggressed against expressed significantly less hostility toward the aggressor than did subjects exposed to neutral pictures or to either neutral or humorous stimuli. The results are interpreted as suggesting that humor and anger responses are incompatible with one another. Gutman & Priest (126) studied the role characteristics of protagonists in aggressive jokes as they influence the judged humor of the jokes. They found that hostile acts committed by a "good person" are judged to be more humorous and less hostile, and that more humor was evoked by a deserving recipient of a hostile act than by an undeserving victim.

Several experiments have explored the effects on aggression-induced physiological arousal of various types of responses. Baker & Schaie (14) found significant reductions in blood pressure resulting from overt aggression by either the victim or by a vicar toward the instigating agent, while pulse rate and respiration were not affected. Indirect expressions of aggression did not reduce arousal. Several studies have shown that counter-aggression serves to decrease anger-induced physiological arousal, suggesting a cathartic effect. Stone & Hokanson (279) suggest, however, that not only counter-aggressive responses, but any response which in the past has resulted in a reduced probability of receiving further aggression will reduce physiological arousal. The implication of this proposal is that "anger catharsis" is an acquired process. Stone & Hokanson showed that intropunitive responses are capable of reducing physiological arousal in aggression recipients. Subjects who learned to self-administer electric shocks in order to avoid a stronger shock delivered by a partner subsequently reacted to the partner's aggression with the intropunitive self-shock behavior, which was accompanied by physiological arousal reduction in the same manner as is the case when extrapunitive responses are administered to an aggressor.

Berkowitz, Lepinski & Angulo (32) have presented evidence that the label which one attaches to his emotional state following anger instigation may also result in inhibition of aggression. Following exposure to an offensive confederate of the experimenter, subjects were led to believe that they were either low, moderate, or high in anger toward the confederate. They were then given the opportunity to shock him. The medium anger subjects were more aggressive than either the low or high anger subjects. Evidence

was presented to suggest that, given the relatively mild provocation, the high angry subjects had become anxious about their "high anger" and that their anxiety had served to inhibit aggressive responses.

Individual differences have received more attention in aggression research than in the other areas of social behavior we have mentioned. Quite often they have been shown to play an important theoretical role. In a study similar to those discussed above of physiological changes accompanying aggression, Gambaro & Rabin (107) showed individual differences in self-reported guilt to be an important variable. Low guilt subjects exhibited a significantly greater diastolic blood pressure decrease than did high guilt subjects following direct aggression toward a frustrator. In a displaced aggression condition in which subjects shocked someone other than the frustrator, low guilt subjects again showed a significant decrease in blood pressure, while high guilt subjects showed a slight *increase* in arousal. The importance of taking the individual difference variable into account is keynoted by the fact that the overall blood pressure change in the displaced aggression condition was *not* significant. *Had guilt not been studied in conjunction with the typical situational variable, the investigators would have been forced to conclude that displaced aggression has no arousal reduction effect.* The authors suggest that "the failure of many investigators to obtain significant results in their evaluation of the catharsis hypothesis may be attributable to the little attention paid to other psychological variables" (107, pp. 93-94).

Feshbach & Feshbach (104) tested the hypothesis that empathy would serve to inhibit overt aggression in children. High empathy 7-year-old males were rated by their teachers as significantly less aggressive than were their low empathy peers; at the 4- to 5-year-old age level, the relationship was reversed. Empathy in females was unrelated to overt aggression at either age level. The results are consistent with an analysis in terms of differential male-female roles and of the role of aggression in the social behavior of males at the two age levels. Levanthal & Shemberg (179) studied the relationship between sex role adjustment, as defined by scores on the Guilford-Zimmerman Index of Masculine Interests, and nonsanctioned aggression. For females, but not for males, poor sex role adjustment was related to non-sanctioned aggression. Masculine females were as aggressive as masculine males. In an earlier study (180), poor sex role adjustment had been associated with inhibition of sanctioned aggression. Taken together the two studies indicate an important interaction between the personality variable and situational factors involving sanctioning and nonsanctioning of aggression. Parsons, Fulgenzi & Edelberg (228) studied aggression in discussion groups as a function of scores on Byrne's Repression-Sensitization (R-S) Scale. Repressors were rated on Bales' categories of behavior as being more aggressive than intermediates or sensitizers while rating themselves as less aggressive than either of the latter groups. This and other studies of aggression which have employed individual difference personality variables clearly sug-

gest, on one hand, the complex manner in which such variables may interact with situational, developmental, and sex variables and, on the other hand, how their inclusion can add to more comprehensive and understandable conceptualizations of aggressive behavior. Kaufmann's (161) book reviews many of these as well as the relationship between aggression and altruism.

COGNITIVE DISSONANCE

Few topics in psychology have received as much attention as the concept of cognitive dissonance. Festinger's model has come in for criticism on both methodological and conceptual grounds (27-29, 43, 63). Bem (29) created something of a furor with his apparent demonstration that observers given descriptions of procedures employed in a number of dissonance studies are able to estimate accurately the attitudinal responses of the actual subjects. Consistent with the results of prior studies, the less the inducement the more Bem's observers attribute attitudes consistent with the behavior of the subjects. Such findings seem damaging indeed to dissonance theory, since they suggest the subjects' behavior in dissonance experiments is governed by external cues and self-perception, rendering unnecessary the positing of an internal "dissonance" state.

Piliavin et al (234), however, have reported results which suggest that Bem's self-perception model may not be capable of accounting entirely for dissonance phenomena. They criticize Bem for failing to simulate dissonance studies in their entirety and argue that his reproduction of dissonance results was due to his selectively simulating certain aspects of the dissonance studies while ignoring other relevant features. Evidence is presented that when the studies are simulated for observers in their entirety, observers are unable to reproduce the attitudinal responses of real subjects. The authors conclude, therefore, that

there must be processes operating within the actual subject in the dissonance experiment that put pressures on him to which the observer is not subject. The contention of the present authors is that these are the pressures stemming from the subject's experiencing dissonance between his initial attitudes and his behavior (234, p. 106).

The theoretical controversy between Bem and the dissonance theorists shapes up as one which has implications beyond the scope of cognitive dissonance per se. Bem's challenge constitutes an attempt to account for behaviors generally seen as determined by internal states by means of a completely behavioristic framework. A study by Kiesler, Nisbett & Zanna (164) provides evidence that when behavior is relevant to belief, people may infer their beliefs from their behavior, supporting Bem's self-perception hypothesis. The question of the relative importance of self-perception and other variables, such as dissonance, in determining attitude change must still be considered an important empirical question (57, 119, 312).

Few studies seem to provide results uniformly supportive of dissonance

theory. One which appears to do so was reported by Doob et al (89). A series of five field experiments showed that when common household products were introduced at a low price for a short time and then raised to a normal price, subsequent sales were less than when the products were introduced at the normal price. This result seems consistent with the dissonance-derived prediction that the more money (effort) expended to attain a product, the more attractive the product will become. However, the authors point out that the results are also consistent with an adaptation level interpretation. Most often, investigators found dissonance theory-predicted results only under specific conditions (e.g. 119, 132, 197, 222).

When we consider the voluminous research which has been conducted within the cognitive dissonance framework, the low frequency with which the contribution of personality variables has been studied is in itself a remarkable phenomenon. This state of affairs is especially puzzling in view of the tremendous inter- and intrasubject variability found within dissonance studies. For example, in an early study by Brehm which is often cited as demonstrating dissonance-reducing devaluation of a rejected alternative, 41% of the postdecision ratings of the rejected alternative were in a dissonance-increasing direction (132). Such inconsistent findings suggest that other variables are also operative in the dissonance situation. It is reasonable to assume that personality differences are among these. They might be expected to influence not only the extent to which a person experiences dissonance, but also the strategies which he employs in order to reduce it. A study by Hamilton (128) provides evidence that this is indeed the case. Need for social approval was found to interact (though not in the predicted fashion) with the mode of response employed to reduce varying degrees of cognitive inconsistency. Love-Hostility scores were also related to differential responses preference, while Dogmatism, Repression-Sensitization, and Dominance-Submission were unrelated to the response variable. Perhaps Hamilton's study is a harbinger of research incorporating the virtually ignored influence of individual differences as they affect dissonance arousal and resolution.

Any given personality test may not be related to how persons cope with dissonance. At the same time it seems likely that theoretically relevant individual difference variables can contribute to our understanding of conflict-resolution and social behavior. Marlowe & Gergen's (199) analysis of the relationship of personality to social interaction recognizes this possibility and presents a thoughtful review of the relevant literature as well as suggestions that will prove valuable to students and researchers. In their paper they deal with the topics of social conformity, the needs for affiliation and achievement, the approval motive, anxiety, self-esteem, social attraction, and social learning. These topics are of special interest to two groups of researchers: (a) investigators who focus mainly on the social situation with personality variables as adjuncts; and (b) those who study situational

variables as a means of validating personality constructs. Marlowe & Gergen reached this conclusion after surveying work emanating from both of these camps:

. . . the consideration of situational variables alone, without respect to the interaction of situation and personality, is a rather fruitless quest. The relationship of personality to social interaction is indeed an intimate one and, if our view of the history of this relationship is correct, the relationship is likely to yield progeny of central significance in years to come.

Marlowe & Gergen's article appeared in Lindzey & Aronson's *Handbook of Social Psychology* (189), consisting of 5 mammoth and well-edited volumes. Volume 3 will probably prove to be must reading for the student of personality, although the others contain many excellent papers.

BEHAVIOR THERAPY

One of the noteworthy developments of the year was the growing application of behavior modification techniques to minor personality deviations. Application of these techniques to deviant behavior got its start, as it were, with mute and otherwise unmanageable psychotics and moderately to severely retarded children. Rewards and punishments were used selectively to strengthen specific adaptive and to weaken maladaptive responses. Interest remains in such knotty problems as those posed by the chronic mental hospital patient (137, 216, 230, 251, 266, 285, 295, 316). Not only does the behavior modification approach go beyond the use simply of reinforcement (e.g. modeling, implosive therapy), but it has also been applied to the everyday "disorders" of life. This area is of interest to the student of personality because it provides a bridge between him and his more clinically oriented colleagues.

Illustrating this are studies of the efficacy of systematic desensitization, or procedures derived from this technique, in reducing test anxiety (68, 76, 88, 148, 196). It appears that reduction of test anxiety, as reflected in subjective reports and positive changes in academic performance, can be effectively accomplished by either individual or group desensitization (148, 196).

The results of one study suggest that the direct experiencing of desensitization is not necessarily required in order for significant reductions in test anxiety to occur (196). Subjects exposed to "vicarious desensitization" in which they merely observed other subjects undergoing systematic desensitization evidenced as great a decrease in self-reported test anxiety, and as great an improvement on a test administered under achievement stress conditions, as did the subjects who directly experienced desensitization. Females showed a significantly greater reduction in self-report test anxiety than did males. The evidence that group and vicarious desensitization is effective in reducing test anxiety, and the fact that relatively few sessions are required even under individual treatment conditions, suggests that these learning-based procedures constitute a practical and economical means of attacking the widespread problem of test anxiety in our schools. This con-

clusion is further reinforced by the report of Donner & Guerney (88) that a desensitization program presented to groups of test anxious female college students by means of tape recordings resulted in significant improvements in GPA and reported test anxiety. Although the taped treatment was most effective when a therapist was present during the five treatment sessions, significant improvements on both dependent variables also occurred in a therapist-absent condition.

The application of behavior therapy techniques to the modification of smoking has not yielded results as positive as those dealing with test anxiety. Aversive conditioning techniques, including the use of such smoking-contingent aversive stimuli as shock and hot smoky air (123) have not proven effective. Some of the studies reporting apparently positive results are difficult to evaluate because they lack control groups (e.g. 213). When adequate follow-up data are collected, treatment effects have tended to disappear (183, 305). In one study (184) an unsuccessful attempt was made to replicate Janis & Mann's (154) finding that emotional role-playing was effective in moderating smoking behavior. Bernstein (33) provided an excellent evaluative review of research on the modification of smoking behavior.

Long-term control of overeating, like smoking, constitutes an area in which behavior modification procedures have had only limited success. Harris (133), however, reported successful reduction of overeating and significant weight losses resulting from a program in which a number of learning-based techniques such as self-administered positive and negative reinforcement, stimulus control of eating behaviors, and modifications in the behavior chain occurring during eating were concurrently employed. The addition of an aversive counterconditioning phase in the programs of a subgroup of subjects made no significant contribution to the program. Although follow-up data were fragmentary, the positive results obtained in this study are encouraging. More systematic assessments of multiple-technique weight control interventions would appear warranted.

The use of learning-based procedures, sometimes implicitly, has been extended into areas such as creativity training (84) and the improvement of individual effectiveness in groups and organizations (81). However, an avenue of inquiry which has thus far been ignored concerns personality variables as possible moderators of outcome. This is understandable since most behavior modifiers have been primarily interested in environmental contingencies and their effects on behavior. However, it is not at all unreasonable to assume that personality variables will be found to affect the outcome of behavior therapy approaches much as they exert effects on the outcome of conventional psychotherapy. For example, there is evidence (85) to suggest that individual differences in the extent to which patients feel that they themselves are responsible for changes in behavior produced by behavior therapy may be relevant to maintenance of improvement.

An excellent illustration of the need to increase traffic on the personal-

ity-behavior therapy bridge is a study reported by Reppucci & Baker (240) on self-desensitization. A group of college students taking a course entitled "Clinical Psychopathology" were shown how to desensitize themselves to one of their own "subclinical" phobias. Prior to this part of the course, the students took a battery of personality tests. Using clinical psychologists' ratings of improvement as a criterion, a number of provocative results were obtained. While there were no sex differences, subjects high on the Hypomania (Ma) and low on the Social Introversion (Si) scales of the MMPI improved significantly more than did the other subjects. The improvers were also significantly higher on managerial and autocratic dimensions. They received lower scores on measures of dependency and masochism. It seems safe to predict that findings such as these will lead to counterparts in the behavior therapy area of the personality explorations concerned with response to psychotherapy (for example, differences between A and B therapists with regard to patient improvement) (314). Now that behavior therapy has achieved the status and recognition it so deserves, it can proceed to an analysis of interpersonal subtleties implicated in its conduct.

INTERPERSONAL ASPECTS OF EXPERIMENTS

Despite progress made in clarifying interpersonal processes related to experimental (and clinical) manipulations, they are still poorly standardized treatments whose replicability is difficult and sometimes impossible. Consequently, linkages between the laboratory and the clinic remain imperfect. Let us turn now to developments in the study of interpersonal processes that influence the outcomes of experiments.

Experimental studies of personality typically involve (a) communications to subjects and (b) interactions between experimenter and subject. Much has been written concerning these facets of the experiment. Yet the personality researcher remains vulnerable to criticism on several very elemental grounds. Why do we specify *personality* researcher? Because personality is the title of this chapter and because personality researchers were among the first to investigate as well as talk about the problem. The personality researcher continues to be more aware of interpersonal confounding than most of his experimental colleagues who study other topics.

The literature of 1969 reflects continued interest in the social psychology of the experimental situation (34, 39, 93, 99). The year was noisily ushered in by a sharp exchange between Barber and his associates (19-21) and Rosenthal (246, 247) regarding assessment of the generality of experimenter bias and experimenter effects. Barber and his colleagues raised a number of critical methodological and statistical issues. These considerations led them to discount the results of a large number of experiments reported by Rosenthal and his associates which purported to demonstrate that experimenters subtly and unintentionally bias their subjects' responses. Five unsuccessful attempts to demonstrate the experimenter expectancy effect were reported.

The statistical criticisms leveled against many of Rosenthal's studies are telling ones, and a majority of the results do not stand up under appropriate statistical treatment. In view of these and other (303, 304) failures to demonstrate experimenter bias effect, it would appear that the phenomenon may not be as general and pervasive as Rosenthal originally contended. On the other hand, Barber & Silver (21) have pointed out that the results of an imposing number of studies are not entirely inconsistent with an experimenter bias interpretation.

It remains for future research to delineate the conditions—probably involving complex interactions between experimenter, subject, and situational variables—under which the phenomenon occurs. As Levy (182) suggests in a commentary on the Barber-Rosenthal exchange, such research will be most meaningful if conducted within the context of theoretical frameworks which address themselves to classes of events within which the experimenter bias effect can be encompassed. From this perspective, the effect may be construed as a special case of interpersonal influence rather than merely as a methodological “spoiler” variable. Theories addressing themselves to interpersonal influence, nonverbal communication, and person perception should than be capable of generating testable hypotheses regarding conditions influencing the occurrence of the phenomenon. The epistemic value of the bias effect would be enhanced in its new role as a vehicle for the testing of theoretical propositions, and the rarity or pervasiveness of its occurrence would assume less importance. One consequence of this approach would be that we would become more prone to speak of experimenter *variables* than of *the* experimenter variable.

Seldom has a piece of psychological research caused a stir in the field of education to equal that brought about by the Rosenthal & Jacobson (249) reports of teacher expectancy effects. Paralleling Rosenthal's laboratory research, teachers were informed that certain randomly selected pupils were “intellectual late bloomers.” These children showed greater positive changes in IQ than in the case of children who were not so labeled. Clai-born (67) reported an unsuccessful attempt to replicate this finding, using the same intelligence measure (Flanagan's Test of General Ability) as that employed in the Rosenthal & Jacobson studies. Not only did the designated “late bloomers” fail to show greater IQ increases than the no-bias control subjects, but there were also no clear changes in observed teacher-pupil interactions.

Working within a different type of setting, Meichenbaum, Bowers & Ross (206) studied the effects of induced teacher expectancies on the academic performance and classroom behavior of institutionalized adolescent female offenders. Six of 14 girls, all taught by four teachers, were identified as “late bloomers.” The situation provided a fortuitous opportunity to examine the expectancies across teachers, and behavioral ratings of the teachers' classroom behavior permitted an examination of individual differences in

manner of implementing them. Compared with control subjects, the "late bloomers" showed significant improvement in performance on objective but not on subjectively scored academic tests. In addition, their classroom behavior improved significantly. Observational ratings of teacher-pupil interactions indicated that the expectancies were not mediated by mere increases in teacher attention to the late bloomers. Rather, there seemed to have been changes in qualitative aspects of the interactions, with some of the teachers increasing positive interactions and others decreasing negative overtures toward the late bloomers. The expectancies regarding these pupils held by the teachers prior to the experimental expectancy induction had no effect on changes in the girls' academic or classroom behaviors, but did influence the manner in which the teachers implemented their new expectancies. Because of its important practical implications, the teacher expectancy effect constitutes a worthy area for increased research emphasis.

A number of important empirical questions remain to be systematically investigated. One concerns the subject's pre-expectancy level of functioning on the dependent variable. How does initial intelligence level, for example, influence the potency of the expectancy effect? Does it influence the manner in which expectancy effects are mediated? Under what circumstances are prior expectancies important? In a sense, teacher expectancy effects may be subsumed under the broader topical area of person perception, and variables identified in social psychological studies of person perception, such as primacy-recency effects and locus of cause, can be expected to apply here as well. Regrettably, the roles of teacher and pupil personality characteristics have so far been ignored. On a methodological level, studies designed to investigate changes in pupil IQ's as a function of induced teacher expectancies have mainly employed the Flanagan Test of General Ability. This test does not appear to be a satisfactory dependent variable measure since strong practice effects occur with retesting. In the Claiborn (67) study, the "late bloomers" showed a 2-month mean increase of 11.5 points, which would be quite impressive were it not for the fact that the control group showed an 11-point gain over the same period. When the subjects have above average initial IQ's, the practice effects serve to effectively lower the performance ceiling, which reduces the probability that a true teacher expectancy effect will show up. An interesting empirical question is whether teacher expectancies are capable of mediating IQ changes on more established intelligence tests, such as the WISC and Stanford-Binet. Factors such as ceiling effects, while of great practical significance in educational situations, may also be crucial in laboratory investigations.

The social-psychological aspects of the relationship between data collector and subject have relevance in a variety of settings outside the psychological laboratory, e.g. the clinic. Rosenthal (248) has summarized a number of studies bearing on clinician expectancy effects in the use of clinical assessment techniques. Sattler & Winget (255) sought to induce expectan-

cies in graduate student intelligence testers regarding the subjects they were to test. They gave the examiners referral reports describing the subjects as average or intellectually superior. The examinees were actually confederates trained to follow scripts in giving their responses. Twelve responses given over the WAIS verbal subtests were ambiguous. The referral reports did not significantly affect scoring of the ambiguous responses. However, more credit was given to the ambiguous responses within the context of other superior responses than within the context of average responses. Evidence that the examiners' own needs may influence their clinical behavior was provided by Masling & Harris (201). Male examiners were found to administer more sexual-romantic cards to female clients than to males, suggesting that voyeuristic needs of the examiner may be gratified in the testing situation. Sex of client did not influence card choice for female examiners. Racial experimenter effects have been reviewed by Sattler (254).

Schultz (259) has discussed a number of trenchant issues relating generally to the human subject and his current role in psychological research. With few exceptions, the experimenter-subject relationship in personality research is formal and impersonal. The subject, generally "conscripted" from an undergraduate psychology course, appears at the appointed laboratory and is run by a methodically efficient experimenter whose obvious interest is in concluding the experiment within the allotted period of time. Under these circumstances, the subject is often requested (and expected) to reveal personally meaningful material about himself, generally with little explanation and minimal feedback. It is difficult to conceive of how we, as students of personality, can accept our methodology as uncritically as we often seem to do.

Two lines of development reflect a lessening of our obsession with "objectivity" and our succumbing to the desire for convenience. One is the respectable status more students are according introspective data (221). To an increasing degree, psychologists are asking subjects about their experience (even if only as a check on the effectiveness of the experimental manipulation). A second line of development, more relevant to our present considerations, is the work of Jourard (159) on the effects of experimenter self-disclosure on subjects' behavior. If the experimental situation is a social situation, it might be expected that its epistemic yield can be increased by maximizing the social aspects of the experimenter-subject interaction. Jourard and his students have performed a series of experiments which suggest that a pre-experimental interview during which the experimenter freely discloses facts about himself results in greater self-disclosure on the subjects' part than is the case under standard experimental conditions. Even as subtle a variable as the amount of eye contact which the experimenter has with the subject has been shown to affect self-disclosure behavior. This research area offers myriad possibilities, not only for further studies of self-disclo-

sure, but also for its interactions with individual difference variables. The potential importance of research in which the social psychology of the experiment is studied in relation to subject self-disclosure is keynoted by Jourard's reminder that all experiments can be conceived of as involving some form of self-disclosure on the subject's part.

A useful anthology that deals with this topic but which, unfortunately, has a misleading title, is Rosenthal & Rosnow's *Artifacts in Behavioral Research* (250). The book contains lucid discussions about the topics of subjects' perceptions of why researchers conduct their psychological experiments, differences between subjects who are volunteers and those who are not, demand characteristics, intended and unintended expectancies, and the details of preparation given to subjects by experimenters. The title is unfortunate because it perpetuates and reinforces the idea that variables such as those implicated in experimenter effects are extraneous. While certain of these effects will be unwanted and extraneous to particular investigations, they are aspects of reality. For personality researchers they are not so much artifacts and sources of annoying confounding as the *raison d'être* for carrying out their studies.

Physical scientists who are impatient with the present state of empirical approaches to human behavior are so not solely because of the elusive quality of the experimenter-subject interaction. It would not be difficult to catalog a lengthy list of nonelusive characteristics of experimental situations that have either been simply neglected or have hardly been referred to. How were the subjects in the experiment recruited? Were they volunteers or were they paid?

Cudrin (80) compared volunteer subjects with nonvolunteers in a prison population and found that volunteer inmates were significantly higher in intelligence. These results cannot be generalized immediately to other groups of volunteer subjects, yet investigations in other subject populations, particularly the college sophomore, have shown the volunteering factor to be important. Barefoot (22), for example, found anxiety to be related to volunteering for psychology experiments. How was the appointment for the session(s) arranged (phone, letter) and by whom (the experimenter himself, his wife, a secretary)? What were the physical arrangements? Was the experiment conducted in a psychology building, in surplus Army barracks, or in an off-campus Charles Addamsish house? What time of day was the subject run? Silverman & Shulman (265) showed that for females (but not for males), amount of time since food intake was negatively related to responsiveness to experimental demands. It may not (or then again it may) make a difference what the temperature and humidity in the experimental room is. What are the frequency and consequences of "leaks" in the subject population regarding the nature of the experiment? Are such consequences at least partly a function of the subject's personality? The challenge is not simply to get rid of confounding and extraneous variables, but also to eluci-

date *why* variable *X* is a source of confounding in situation *A*, but not in situation *B*.

The question of the standardization of the psychological experiment may in some ways prove to be analogous to that of protecting the rights of the subjects who serve in them. Nightmarish terms such as "informed consent" did not emerge from researchers' soul searching. Changes in public policy, induced largely by a small number of documented embarrassments, have forced behavioral researchers to revise the balance between what is desirable for the sake of the experiment and for the sake of the subject. Having made that revision this question remains: Granted that it is essential that the rights of subjects be protected, what effect will requirements such as that for informed consent have on the pursuit of psychological knowledge? The medical profession has long faced the dilemma posed by the need to avoid inconvenience or harm to patients on the one hand, and the desire for maximization of scientific knowledge on the other (163). The behavioral sciences can no longer avert attention from it. There seems little question but that the field of personality will cope with it in some way. However, what consequences this adaptation will have are not yet clear. Stricker, Messick & Jackson (282) have discussed several avenues open to researchers who rely on experimental designs which call for some type of deception of subjects.

A CONCLUDING COMMENT

We have chosen to explore the forest with perspective, but also with several selected close-up looks. Our excursion is over an ecosystem that has many elements. What integrates these elements is the goal of accounting for the distinctiveness of the behavior of individuals. We have all pored over the exhaustive and frustrating lists of definitions of personality. They can be truly bewildering, especially for a newcomer. Yet, anomalously, more and more newcomers are visiting and homesteading in this forest of personality. How might this migration be explained? The answer, we feel, is to be found in the tantalizing quality of individuality. Why to we behave as we do? Can the personality psychologist account objectively for individual differences in, and predispositions toward, behaving in certain ways? With each passing year, the answer to the second question is a stronger affirmation. This is true despite seemingly slow progress in answering the "why" question.

In our view, the science of personality revolves around the study of individual difference variables and behavioral predispositions which underlie trans-situational consistencies in behavior. As a scientific discipline a rapprochement is required between two concerns from which modern psychology evolved: (*a*) concern with individual differences, typified by the work of Galton and the early psychometricians; and (*b*) concern with the discovery of nomothetic behavioral laws through manipulation of environmental

variables and use of experimental methods, typified by the early experimental psychologists. Cronbach's still refreshing 1957 paper (77) represents one of the major contributions to the recognition of this need. We agree with Cronbach that neither emphasis in and of itself is complete. The most productive approach to the study of personality must consider the manner in which individual difference variables interact with situational factors in determining behavior (253).

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CLASSIFICATION OF THE BEHAVIOR DISORDERS 171

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INTRODUCTION

In the field of classification of behavior disorders, the last 4 years¹ have been a period of ferment, controversy, and innovation. The subject of diagnosis, once regarded as a preserve of traditionalists, has acquired for many workers in the field a new excitement and vitality. The assumptions, models, divisions, and objectives of psychopathological categorization are being actively scrutinized. At the same time, the traditional conceptions of psychiatric diagnosis show no dearth of articulate defenders. A plethora of suggestions for abolishing, rethinking, and revising diagnostic practices and classifications contrasts sharply with the recent second edition of the *Diagnostic and Statistical Manual* of the American Psychiatric Association (6, 230), a document that affirms the traditional postulates and concepts of diagnostic description. This clash of outlooks impels us to devote more space, by comparison with our three predecessors (92, 137, 271), to the several contending formulations of the assumptions and models underlying diagnostic activity. By contrast, we will refrain from systematically considering the diagnostic usefulness of psychological tests and the experimental comparison of nosological categories, two specialized and intensively investigated topics which can no longer be satisfactorily treated within the scope of this review.

GLOBAL OBJECTIONS TO THE SYSTEM AND MODEL OF DIAGNOSIS

Three currents of thought as different as existentialism, behavior therapy, and the community mental health movement have given rise to theoretical arguments that favor rejecting a number of basic tenets of traditional psychiatric diagnosis: the belief that a hidden structure underlies the observable phenomena of psychological dysfunction, that the disorder observed inheres in the individual afflicted, and that this affliction is basically

¹Literature covered in this review encompasses the 4-year period from January 1, 1966, to December 31, 1969. In a certain number of cases, earlier writings have been included for the sake of continuity.

similar, though not necessarily identical to disease in the medical and biological sense of the term.

Laing (121, 122) is probably the most outspoken critic of conventional diagnosis from the existentialist point of view. Writing primarily about schizophrenia, but with potential application to the entire range of traditional psychopathology, he sees in diagnostic labeling a dehumanizing, anti-therapeutic activity that widens the gap between the observer and the observed, cripples the interpersonal involvement of the parties to this transaction, and results in pernicious, self-fulfilling prophecies. To paraphrase in simplified form Laing's complex and subtle argument, schizophrenics (and by implication other patients as well) behave as they do because they are treated as schizophrenics. The point that Laing makes is not entirely new; other writers, including several during the period of this review (e.g. 20, 117, 198, 199, 209, 213, 245, 261) have called attention to the power of the diagnostician to shape the role and the behavior of the patient in order to make it conform to the diagnostic stereotypes and have cautioned against the self-fulfilling nature of predictions based on diagnostic labels. Laing has gone further in proposing and implementing an alternative to the traditional, asymmetrical assignment of diagnosis by the expert to the patient in the form of individual understanding of each help-seeking person in the context of an intense, bilateral social encounter.

From the other end of the theoretical spectrum, proponents and pioneers of behavior modification (9, 51, 99, 254) have attacked the traditional diagnostic scheme which, in their view, is inextricably bound up with the "medical model." Rejecting all hypothetical, underlying, unobservable structures of either the biological or psychodynamic variety, these investigators proclaim that the symptom is the disorder. Although they advocate the collection of pretreatment information, their diagnostic activity differs markedly from the traditional anamnesis. What is sought is a catalog of symptoms and behavioral deficits, together with the reinforcement contingencies of their occurrence. These data are both necessary and sufficient for instituting an individually geared behavior modification program; traditional diagnostic categorizing is superfluous. Dimensional constructs are eschewed, comparison across subjects de-emphasized; each case comes to be a new experiment with an *N* of one. At this point, the *modus operandi* of the behavior therapist converges with that of the existentialist; the individual is paramount, the category trivial. The difference lies in the existentialists' stressing empathy and sharing of perspectives; the practitioners of behavior modification emphasize control of behavior through reinforcement.

Their focus of thought and observation is upon two planes: the general laws of behavior applicable to all humans and the past and present conditions of reinforcement peculiar to the subject in question. Between these two levels, little room remains for categories or dimensions that set apart some subjects from others.

The third position that has taken a critical and searching look at the diagnostic enterprise arose as part of a more general "explosion of discontent" (86) with traditional roles and practices of the mental health professions and with the nonpragmatic and ill-fitting nature of the traditional diagnostic categories for the new modes of application outside the hospital and the office. Albee (2) has been particularly articulate in calling for new models and concepts that would dispense with the medical heritage implicit in the term "mental illness" and with its manifold divisions. Instead, concepts derived from social psychology and psychology of learning should be applied to the problem of living in the social environment. Consonant with this position are the formulations by Scheff & Scheff (213) and Newbrough (173), who attempt to integrate psychopathological classification with concepts drawn from the sociology of deviance. Albee (2) has also proposed a reversal of emphasis from a preoccupation with the pathological and the bizarre in the lives and actions of help-seeking clients to a consideration of their strengths and skills. A similar theme is found in the work of Phillips (185), who maintains that social competence is more appropriate criterion for the evaluation of effectiveness in community mental health programs than the traditionally invoked indices of diagnostic remission, symptom removal, and subjectively rated improvement. Several other investigators have accorded a pivotal role of the concept of social competence as a criterion of adaptation and adequacy (19, 63, 67) and empirical applications of this concept have been reported to psychiatrically disabled (187, 202, 218), general normal adult (67), college student (67), and grade-school pupil (116) populations.

The thrust of the three positions just presented is selectively directed at several aspects of diagnosis. Yet all of the points of view are at one in rejecting or minimizing the role of traditional diagnostic designations. All are skeptical concerning the applicability of the term "mental illness" to the phenomena of maladaptation. Instead of being an externalization of a covert structure, maladaptation is behavior and life itself. At a higher plane of inference, another commonality can be detected; the exponents of the three positions stress the modifiability of behavior and its ever-changing character over space and time. They reject the concept of the individual's inextricable entanglement in a nexus of biological or psychosexual origin. Hence, importance is accorded to the person's interaction with his environment; flux is emphasized and stability de-emphasized. Diagnostic activity transcends the individual and seeks information on his interplay with the milieu. Finally, the three positions share their origins in clinical observation and therapeutic practice. This sets them apart from two additional critiques of conventional diagnosis whose concern is mainly with the philosophical premises and legal-political consequences of the notion of mental illness.

To Sarbin (209-211) the concept of mental illness represents a reified metaphor and a logical absurdity, denoting the disease of an entity that does

not exist. The difficulties with this concept, however, are, not exclusively philosophic; the practical consequences of the application of the concept are both misguided and unfortunate. Among these results is the social rejection of the "mentally ill" as uncontrolled and irrational beings, a onesided search for the causes of their dysfunction in inner experience rather than in external social reality, and a slavish adherence to the medical model of cure. As an alternative to viewing dysfunctional social behavior as evidence of "mental illness," Sarbin (210, 211) proposes to view these manifestations as "transformation of social identity" associated with social degradation and loss of status.

Sarbin's formulations have a number of points of contact with the point of view of Szasz (e.g. 245) for whom the notion of "mental illness" is but a misnomer for those "problems of living" on which the society prefers to defer or forego rendering an ethical or legal judgment. The elements of game-playing, role-enactment, and decision-making are, according to Szasz, inevitably present in mental illness and absent in the physical disease that served as the model for it. In reference to diagnosis, Szasz does not exclude outright the empirical and substantive basis of nosological differentiating activity. However, it is his contention that the application of diagnostic designations inevitably constitutes an illegitimate exercise of social power with adverse consequences for the person diagnosed.

DEFENSE OF TRADITIONAL CLASSIFICATORY MODELS

As already indicated, there has been no paucity of voices raised in defense of the conventional model of classification and diagnosis. This activity has taken two forms. First, a considerable number of articles have appeared, arguing for the validity and usefulness of traditional models and divisions of diagnosis. Second, in a less direct way the appearance of a revised diagnostic manual (6), strongly beholden to traditional notions as to the nature of conditions to be diagnosed, bespeaks the vitality and sturdiness of medically inspired diagnostic effort. We propose to present first a number of arguments in favor of retaining classification as it has been traditionally applied in order to proceed then to a more detailed presentation of the new diagnostic guide and of the reactions to it.

The advantages of traditional diagnosis.—The defenders of conventional diagnostic activity have been concerned with a number of issues. Arguing against Sarbin, Ellis (40) has stressed a number of valid features in the mental illness analogy: the helplessness of the client, his suffering, his need for expert intervention. Moreover, he maintains that it is not the label that causes the public to reject the mentally ill. Consequently, he proposes that resources and time be spent in changing the rejecting attitude rather than in finding a new name for the phenomenon. Grinker (70, 71) defines illness developmentally as a rigidification of behavior patterns beyond adaptive usefulness and sees no empirical basis for choosing a unitary, whether so-

cial or biological, explanation for the phenomena of maladaptation. In the absence of such information, he suggests that the notion of disease be retained, as expressive of the psychophysiological unity of the adaptive struggle. In Great Britain, Roth (203) recognizes the imperfection of the diagnostic system as it now exists, but upholds the functional value of categories that are defined on the basis of clinical values. He suggests refining the available instruments and categories rather than dismantling the imperfect structure of diagnostic knowledge and starting anew. Writing within the tradition of French psychiatry with its somewhat different classificatory system, Abély (1) stresses the virtues of flexibility and adaptability within the current diagnostic scheme and is skeptical about any new systems brought forth to replace it. Like Roth, he too endorses growth by modification and accretion. Another French psychiatrist, Marchais (149-152), concedes that a medically oriented psychiatry has so far not progressed beyond the isolation of syndromes; no disease entities with a triad of common etiology, symptomatology, and treatment indication have as yet emerged. Nonetheless, he proposes to solve this dilemma by objectifying diagnostic signs, separating observation and inference, and supplementing traditional diagnostic activity by a functional analysis of symptoms and models. All of these objectives can be accomplished without scrapping the model of mental illness and without rejecting its nosology. In this country, Ziskind (270) attacks what he believes to be widespread misconceptions concerning the medical model; its application does not presuppose either a unitary cause or the complete knowledge of etiology. In this view, the state of knowledge concerning causative and precipitating factors has sufficiently progressed to replace symptomatic classification with an etiological one. The German psychiatrist, Weitbrecht (263), justifies his defense of the disease model by referring to the nonspecificity of origins of psychopathology, especially in the area of psychosis. According to Weitbrecht, many of the symptoms and syndromes can arise on either a psychological or a somatic basis; this justifies the expectation that the psychogenic disorders may be biological disease entities in disguise. However, the most outspoken defenders of the disease concept are to be found among the biologically oriented investigators of schizophrenia, such as Bender (14), Fish (55), Heath & Krupp (85), Osmond & Hoffer (178). Without explicitly extending their arguments to the entire gamut of clinically recognized maladaptation, these authors regard schizophrenia as a disease in the concrete, empirical sense of the term, one with a biological etiology, psychological manifestations, and biological treatment indications. This orientation is expressed in the foundation of a new *Journal of Schizophrenia* which, to paraphrase from its opening editorial (177), is predicated on taking seriously the dictum that schizophrenia is like any other illness. Furthermore, the almost simultaneous appearance of a journal similar in name and orientation in Russia² suggests that this current

² The reviewers have not yet seen the actual issues of *Zhurnal Shizofrenii*.

trend of thought continues to be worldwide. More generally, we have gained the impression from our survey of the writings of diagnosticians over the last 4 years that the traditional conception and nosology is accepted by the majority of clinical and research workers in the several fields concerned with maladaptation. Despite the several critiques presented earlier, the majority view of the professions concerned endorses some variant of the disease concept, although wide room remains for disagreement as to the substantive versus metaphoric use of the term. Accordingly, it is not surprising that the updating and revision of the *Diagnostic and Standardization Manual* of the American Psychiatric Association has resulted in reaffirming and restating these conventional and widely held concepts. We now turn to the consideration of the features of this document and of the reactions that it has evoked.

The current version of the diagnostic manual.—Perhaps the most important single psychiatric event of the last 4 years has been the appearance of the second edition of the DSM-II in 1968. On a first and cursory reading of this revision, the reader is immediately struck by the similarity of the current version to the original published in 1952 (5). Nonetheless, there are differences of general orientation and classificatory detail which, with the help of the manual itself and the explanatory article by Spitzer & Wilson (230), we would like to summarize. On the plane of general rationale, the manual parts with the notion of "reaction," pivotal to its first edition, and limits itself to names of the disorder, such as schizophrenia, depression, or neurosis. In organization, the number of broadest, first-order categories is increased from three to ten. Turning to more specific features, one notes an increase in the number of subdivisions of schizophrenia, a more detailed classification of mental retardation in both kind and severity, the addition of a detailed taxonomy of behavior disorders of childhood and adolescence, the reintroduction of the category of hysteria, and the division of alcoholism into a larger number of specific syndromes. It is apparent that, by comparison with the first edition, there are many additions and few omissions. Its most noteworthy eliminations concern the sociopathic personality disturbance, no longer listed as an entity. One of its subdivisions, dyssocial reaction, is assigned to the residual category of conditions without manifest psychiatric disorder, along with marital, occupational, and social maladjustments. On a higher plane of generalization, it would not be unfair to characterize the new manual as more traditional in general rationale, more detailed in specific and general categorization, and more restricted in its overall scope. In reference to the last feature, we are particularly struck by its distinction of psychiatric disorder as a subset of the broader category of maladjustment that is implicit in some of the features of the classification presented, although this is not being explicitly stated in reference to concrete criteria.

Criticisms and reactions to the manual have not been slow in appearing.

By now a wide range of professional comment is available. On the positive side, there are authors (11, 112, 128) who praise the clarity of syndrome delineation and the marked decrease in etiological inference. Its critics object to classification based on symptom detail instead of severity of disorganization (157); the omission of the concept of reaction, with the attendant if unspoken assumption of illness (255); the lack of provision for recovery from at least some of the conditions listed, e.g. schizophrenia (225); and the proliferation of detail resulting in what one of the critics (94) has called the "labeling syndrome." Other criticisms are opposite and contradictory in their thrust. While some of the writers castigate the acceptance by the authors (153) of the manual of the anxiety-defense theory of neurosis, others (e.g. 266) decry a diagnostic classification that is dependent on symptoms and that disregards commonalities at a deeper level of psychodynamics. Finally, there are objections to detail. While the classification of mental retardation has been applauded (52, 94), the taxonomy for the area of children's behavior disorders has been severely attacked (52, 53) as being logically unsound, pragmatically difficult of application, and based on a confounding of social, legal, and psychological concepts. Similarly, there are clinicians (246) who regret the omission of several varieties of personality disorder and wish for the inclusion of psychotic reactions outside of the categories of schizophrenia and affective psychosis.

To these published comments, we would like to add some of our own. Upon examining this manual, we are struck by the contrast between its orientation and rationale and that of the current effort in research and innovation in the helping professions. There is little evidence, for example, of the categorization in the manual being influenced, except in the case of children's disorders, by the imposing store of information emanating from factorial research. Similarly, the breakdown in the manual only poorly accommodates the extension of mental health services beyond the groups of conventional psychopathology. While the field of activity of the several professions has expanded, the range of the conditions included under the heading of psychiatric disturbance has experienced a slight degree of shrinkage.

Having voiced these criticisms, we recognize the constraints under which the authors of the manual have operated. The most important of these was the objective that they set themselves—to bring American diagnoses in line with the current edition of the World Health Organization's *International Classification of Diseases*. The second constraint is inherent in the committee format of deliberation which served as the basis for the development of this manual. One of the participants in this task (76) has referred to the compromise decisions that this approach inevitably engendered. Finally, the pressures of tradition are not to be discounted; it would be unrealistic to expect an official diagnostic system to break new ground and to interrupt continuity with the past. For more innovative proposals, the worker in the field must turn to individuals free to choose their own framework and to implement it on the basis of their own predictions.

PROPOSALS FOR NEW SYSTEMS OF CLASSIFICATION

Suggestions for new or revised modes of classifying maladaptation have come from a variety of sources. Most of these proposals have been animated by a search for a general, guiding principle of classification that is lacking in the official systems in this country and abroad. Apart from this general feature, the formulations presented differ widely in the degree of change sought, the objective of classification introduced, and the theoretical allegiance demonstrated.

Within the framework of traditional descriptive psychiatry, a number of authors, American and European, have endeavored to subordinate the familiar components of diagnostic classification to one or more explicit continua. Barahona Fernandes (12) has formulated a fourfold scheme based on syndrome manifestation, course, intensity, and etiology. In the proposal by Ziskind & Jacobson (270), the role of etiology is paramount and biogenic, psychogenic, and sociogenic syndromes are sharply distinguished. Etiology is also a prominent feature in Leonhard's (130, 131) proposed nosology which, among other things, differentiates wish and fear neuroses, exemplified by hysteria and hypochondriasis, respectively, and sets apart integrated and confused varieties of schizophrenia, believed to be traceable to structural nervous system deficit in the former, and metabolic deficiency in the latter case.

On a more novel theoretical basis, Foulds (56) proceeds from the general postulate of the existence of one "personal illness" that encompasses all clinical manifestations of maladaptation. A feature of Foulds' position is the emphasis placed upon the discontinuity between personal illness and normal adaptation. Symptoms and traits are distinguished with equal sharpness. In Foulds' view, the crux of all psychological affliction lies in the person's impaired ability to engage in social relationships. Within this framework, the four conditions of personality disorder, neurosis, integrated psychosis, and nonintegrated psychosis become progressive downward steps on the road to social incapacitation. Distinctions beyond these four general points of reference are a matter of transformation of personality traits into symptoms, eventuating in such contrasting syndromes as obsessive-compulsive and hysteric neuroses.

A considerably greater degree of complexity is inherent in Thorne's (249, 250) existentially inspired taxonomy of maladaptive psychological states. The reviewers despair of their ability to summarize this highly detailed scheme within the present limitations of space. Suffice it to say that Thorne aims at providing an inclusive catalog of existential loci of disturbance resulting in deficiencies in levels of integration and nonattainment of important personal goals. Sources of these loci of disturbance, e.g. constitutional, developmental, role-determined, are taken into account in ordering an imposing list of specific maladaptive personality types. It is not clear whether Thorne's multidimensional classification is designed to supplant or only to

supplement the more familiar diagnostic designations. In these reviewers' view, Thorne's taxonomy is strongest in covering in considerable detail the ground skirted or neglected in the more conventional systems—that of sub-clinical disorders of living on the periphery of traditional psychopathology. Time will tell how practically and heuristically useful Thorne's unusual and ambitious work will prove to other workers in the field.

Worthy of mention are the attempts within the psychoanalytic movement to formulate a diagnostic scheme based on Freudian concepts. Several observers (19, 184, 260, 275) have recognized the poor fit between Kraepelinian diagnosis and psychodynamic conceptualization which, in the actual analytic situation, causes psychoanalysts to experience difficulty, or to lose interest, in applying standard diagnostic nomenclature. The three schemes to be presented appear to arise from this dissatisfaction and to respond to the need for a classification indigenous to the conceptual framework of psychoanalysis. Glover (64) classifies psychological dysfunction on two axes: (*a*) stage of psychosexual development at which the client is fixated, and (*b*) relative predominance of the mechanisms of introjection and projection. Employing both Freudian and Adlerian concepts, Bour (18) proposes diagnosing individuals in reference to three sets of characteristics: 1. the vicissitudes of aggressive, sexual, and power impulses; 2. the psychosexual developmental stage attained; and 3. the psychodynamic mechanisms prominent in the disorder. Finally, reference should be made to the multidimensional diagnostic index developed under the aegis of Anna Freud (e.g. 248), first for standardized psychoanalytically based description of children, and more recently for that of adults.

Within the adaptational frame of reference, Phillips (185) has taken the symptom as the point of departure. The resulting classificatory scheme is two-dimensional: symptoms are grouped according to their social character, or role, and their mode of expression, or sphere. Dominance patterns in each of the three roles (turning against self, turning against others, avoidance of others) and four roles (thought, affect, somatization, action) are established by objective, statistical means. Quite in contrast to Foulds, Phillips emphasizes the continuity between disorder and baseline adaptation. On this basis, symptom dominance patterns are thought to be related to the person's level of development achieved and to his customary life style. Empirical evidence that has accumulated links above all role dominance in the category of turning against self to high and complex modes of preclinical adaptation; turning against self and avoidance of others are associated with lower levels of premorbid attainment. Phillips' formulations and findings are quite compatible with Feffer's (50) theoretical position that views all psychiatric symptoms as results of faulty or incomplete perceptual decentering, even though Feffer has explicitly desisted from constructing a hierarchy of symptom manifestations based on several kinds or degrees of failures of decentering. This task remains to be tackled in the future.

WORK WITHIN THE CONVENTIONAL DIAGNOSTIC SYSTEM: THE DIFFERENTIATION AND IDENTIFICATION OF SYNDROMES

The sights of the authors just reviewed have been set on a revision of the diagnostic system as a whole. In contrast to it, the work of the investigators now to be presented aims at a more specific objective: that of delineating, by clinical or statistical means, empirical patterns of maladaptive behavior. Some of this research has focussed upon the differentiation of syndromes within the broader diagnostic categories, the identification of new entities of psychopathology, and the resolution of dilemmas posed by "borderline" or "atypical" cases that fit poorly into the traditional classificatory grid.

Syndromes within diagnostic categories.—One of the most active topics of investigation in this general area is that of process vs reactive varieties of schizophrenia. We see no reason for reviewing this accumulation of work in detail since no fewer than four evaluative articles (59, 87, 88, 100) have appeared on this topic in the last 4 years. However, it is worth noting that the attention to this continuum has clearly eclipsed the interest of research workers in the traditional varieties of schizophrenia. A recent annotated bibliography (216) documents the prominence in English-language journals of three axes of schizophrenia: process-reactive, acute-chronic, and paranoid-nonparanoid. The independence of the process-reactive and paranoid-nonparanoid breakdown has been studied by Goldstein, Held & Cromwell (68). A significant, if nonlinear, relationship has been established; reactive (or "good premorbid") schizophrenics are typically paranoid while process (or "poor premorbid") schizophrenics have an almost equal chance of falling into either paranoid or nonparanoid category. Other features of the process-reactive axis that have been investigated concern symptomatology (27, 28), cognitive style (30), and outcome (29). While the association of this variable with outcome is not novel, it is worth juxtaposing these positive results, as Garmezy (59) has done, with the indeterminate relationships that have emerged with the classical, Kraepelinian divisions of schizophrenia. As far as symptomatology is concerned, Cancro & Sugerma (29) have demonstrated high concordance between a novel, symptom-based criterion of process vs reactive schizophrenia (Bleuler's primary symptoms in the former, secondary in the latter case) and more conventional indices of premorbid attainment in case history data. That a curvilinear relationship obtains between the process-reactive dimension and the cognitive style of psychological differentiation (30) further attests to the broad range of correlates of this variable, some of which apparently are far removed from manifest pathology. On the basis of all of these observations, we concur with the reviewers of research on the concept (59, 87, 88, 100) in concluding that the process-reactive dimension is a fruitful one for accommodating a number of important distinctions within schizophrenic populations. Work

done so far suggests that the characteristic is distributed as a dimension and not as a dichotomy. To this observation we would like to append the point made by one of us (186) earlier: the process-reactive continuum in schizophrenia represents but a special case of premorbid attainment, or social competence, shaping onset, manifestation, course, and outcome of psychopathology. More on this will be said later in the appropriate portion of this review.

The problem of differentiating syndromes within the realm of depressive and affective disorders has been almost as active as the one with schizophrenia. The thrust of this effort, however, has been quite different. Whereas most of the research on the division of schizophrenia has been concentrated on a few specific dimensions, the work on the components of depression has been characterized by a more far-ranging search, by a variety of methods and with a variety of specific objectives. A noteworthy feature in this area of investigation has been the appropriate application of a number of sophisticated statistical techniques, a trend worthy of emulation in many other areas of diagnostic subclassification.

Kendell's (106-108) research program illustrates the use of empirical means to solve problems in diagnostic differentiatism. His concern was with the independence of reactive and endogenous depression. Although he succeeded in isolating third-order factors corresponding to the diagnostic stereotypes of the two conditions, he cautioned against inferring independence of these two syndromes. Discriminant analyses failed to differentiate the two varieties of depression as few patients conformed to either type in pure form. Kendell's conclusion points to the mixture of these two components of depression in actual cases, resulting in their continuous rather than bipolar distribution. In contrast to Kendell's work, research on the differentiation of bipolar (manic depressive) and unipolar (psychotic depressive) divisions of effective psychosis, pursued independently by Angst (7) in Switzerland and Perris (183) in Sweden, has yielded both positive and convergent results. The investigators concluded (8) that the two types of affective disorder could be differentiated on the basis of premorbid personality, number and duration of psychotic episodes, prognosis, and the weight of an hypothesized hereditary component. The methods of these European investigators differed from those of Kendell as did the conditions investigated; it would be interesting to mutually enrich their procedures and objectives by proceeding beyond symptom manifestations in Kendell's case and by restricting the consideration to the symptoms in the case of Perris and Angst. Implementation of such a research plan would shed light on the independence or interrelation of the several criteria by means of which the investigators have chosen to determine the existence of separate syndromes.

Outside of the distinctions incorporated into the diagnostic classification, Blinder (16) has proposed a fivefold classification of depression into physical retardation, tension depression, schizoaffective depression, depression secondary to problems of living, and depression as a prodrome of "occult

organic lesions." According to Blinder, this fivefold division has important implications on the planes of management, treatment, and prognosis. Yonge's (269) proposal offers an epigenetic trichotomy of depression, with the term "autochthonous" reserved for states originating in the earliest period of life, "abreactive" traceable to later periods of life, and "alloquial" occurring essentially in the context of hysterical personality.

Factorial studies, such as those by Raskin and collaborators (195-197) as well as others (80, 93, 144), have been useful in establishing the components of depressive experience which do not, however, necessarily correspond to diagnostic varieties of depression. Specifically, Raskin's factorial investigations, based on interviews, ward behavior ratings, and self reports, identified the dimensions of depressive mood, feelings of guilt, hostility, anxiety-tension, cognitive loss, lack of interest in activities, somatic complaints, sleep disturbance, retarded speech and behavior, bizarre thought, excitement, and denial of illness.

In the light of the substantial amount of work in the areas of psychosis, we have been surprised not to find any substantial amount of evidence of work concerned with the differentiation of nonpsychotic syndromes. While descriptive, clinical accounts of several of the varieties of neurosis or character disorder are well represented in the professional literature (e.g. 22, 32, 58, 77, 90, 109, 119, 181, 200), interest in delineating on an empirical basis these varieties of psychopathology appears to be clearly lacking. It would suggest that investigators in the field attach little value to these kinds of specific variables as the burden of research is concentrated on distinctions that can be made within the area of psychosis.

New syndromes.—By contrast, there is no dearth of articles describing new diagnostic entities outside of the currently valid diagnostic classification. One group of disorders so introduced could be described under the heading of "psychopathology of our times." "Existential neurosis" (147) characterized by the triad of meaninglessness, apathy, and aimlessness, "protest psychosis" (23) occurring in the form of mutism and rejection of the white majority culture in ideologically militant black individuals, and the "neurosis of conscience" (13), defined on the basis of immaturity with dependence on an ego-foreign authority, are three examples of this trend. Documentation of these syndromes is at this point in the form of clinical description; it is an open question whether these entities will stand up on scrutiny by more rigorous means. Nonetheless, the descriptions extant raise the intriguing question as to the plasticity of psychopathological manifestations in response to social and historical pressures. To what extent, it can be asked, may patterns of maladaptation be described without reference to the social and historical influences that impinge upon individuals at a given point in time? to what extent does each social and historical force shape its concurrent psychopathology?

A less novel concern of writers in this area is with patterns of psychiatric disorder that many clinicians recognize even though no explicit place is provided for them in American or international classifications. Prominent among these is hysterical psychosis, overlapping with the category of "sensitive reference reaction" as described in a posthumous monograph by Kretschmer (118). Hysterical psychosis has been in the forefront of psychiatric interest (89, 123, 268), especially in the light of transcultural observations on its manifestation. Some observers feel, however, that the occurrence of this disorder is not limited to faraway places (123, 268) and that a distinct need exists for providing a niche in the current psychopathological taxonomy for a psychotic disorder that is culturally shaped in its manifestations, conspicuous and dramatic in its appearance, and benign and transitory in outlook.

Other disorders whose mention we have found in the literature include acute pernicious psychosis which a clinical investigator in the Netherlands (251) observed in over 20 cases. This condition remains obscure in its origins, but is dramatic in its manifestations and lethal in outcome, even though unaccompanied by gross structural change, unless it is arrested by nonspecific somatic treatment such as electroconvulsive shock. Of equally obscure etiology is the intermittent twilight state described by Müller in Germany (161).

Nonspecific conditions.—Apart from these disorders, we have noticed a considerable amount of attention paid to two conditions marked by their nonspecificity. The former is the borderline syndrome (72, 110, 136, 262), combining the features of neurosis and psychosis; the latter has been named the social breakdown syndrome (182, 204, 274). Grinker et al (72) have applied the techniques of factor, cluster, and discriminant analysis in order to identify the components and to describe their features within this ambiguous category. Their effort again is perhaps more important for the application of statistical procedures in the resolution of a difficult diagnostic problem than it is for the features of the varieties of the borderline syndrome that were identified.

The social breakdown syndrome has come to the attention of the helping professions as a challenge to the existing modes of treatment. A varying if sizeable percentage of the psychiatric patients admitted became long-range residents of the public psychiatric institutions. In their chronic state, their diagnostic differentiating features became blurred and their final characteristics are believed to owe more to the impact of the institutional environment than to the original disorder that led to their admission (182, 204, 274).

Finally, we cannot omit mention of the diagnostic dilemmas posed by the sequelae of some of the tragedies of our times: the psychiatric disabilities of some of the Nazi concentration camp survivors (e.g. 38) and the victims

of atomic bombing of Japan (133). For purposes of this review, we can only note that the conditions resulting from these catastrophes fit only with difficulty into the categories provided in the traditional psychiatric schemes.

**PSYCHOPATHOLOGY IN CHILDREN:
A SPECIAL PROBLEM OF CLASSIFICATION**

The span of the last 4 years has been an eventful one for the classificatory enterprise in child psychopathology. At this point there are, in addition to the sections on children in the revised manual of the American Psychiatric Association, at least three inclusive schemes that have been individually or collectively proposed: the classification formulated by the Group for Advancement of Psychiatry (75), as well as those prepared by Rutter (205) in England and Mackay & Bigras (146) in Canada. All of these efforts are animated by the objective of constructing a classificatory scheme that is proper to the disorders of childhood and is not a mechanically applied replica of categorization of disturbed adults (160). The scheme developed by the Group for Advancement of Psychiatry is based on the concepts of developmental stage, the degree of stress, and the appropriateness of response to it. This results in a continuum from healthy through reactive, developmental, and neurotic to psychotic dysfunctions; psychophysiological disorder, brain damage, and mental retardation remain outside this continuum, but are explicitly recognized as separate diagnostic categories. To Mackay & Bigras, the unifying concept is that of structure; the entities proposed fulfill the requirement of being based on descriptive, dynamic, and structural similarities. The groupings proposed are not ordered in a developmental hierarchy. Rather they are defined on the basis of commonality of qualitative features. Thus, sexual perversions and disorders due to antisocial transgressions are recognized as discrete categories; disorders of adaptation, hyperactivity, and character are specifically distinguished from each other. Rutter's approach is, by contrast, much more empirically based; it proceeds from recognized syndromes rather than from rational considerations. Such categories as the hyperkinetic child are included, and room is provided for educational retardation as a category, independent of either intellectual, developmental, or neurotic dysfunction. Rutter's scheme is perhaps the least reminiscent of the three of the prevailing classifications used with adults. It is also noteworthy for its author's insistence that the disorder be diagnosed and not the child.

Apart from these general proposals, we would like to mention briefly the work of Fish et al (54), who addressed herself to the difficult task of developing a diagnostic nosology for schizophrenic children under the age of 5; the differentiation of subtypes of childhood schizophrenia by Goldfarb (66); and to refer to the empirical research program by Jenkins (96-98) which has resulted in the identification of five factorially based groups of ambulatory psychopathology characterized by shyness, anxiety, hyperactiv-

ity, unsocialized rebellion, and socialized delinquency. The characteristics of these five groups are empirically linked to distinct socialization patterns. Several of the features of Jenkins' findings have been incorporated into the revised diagnostic schema by the American Psychiatric Association.

**FROM SYMPTOMS THROUGH SYNDROMES TO SCALES:
NEW TRENDS IN DIAGNOSIS**

Symptoms.—Three currents of activity have united in reviving interest in the symptom as a cornerstone of psychiatric classification. The proponents of behavior modification, as already indicated, see in it the only observable and real expression of disorder. The users of multivariate techniques proceed from the symptom as a point of departure, while the adaptational point of view accords it the important role in the triad of premorbid functioning, precipitating stress, and symptomatology. Yet it is paradoxical that the discrete symptom, by itself, carries little predictive value (186). Consequently, an important share of research in this field has been directed at sifting and grouping symptoms on a theoretical or empirical basis in order to relate them to diagnostic, prognostic, and other criteria.

Although the factorial approach has been traditional for the identification of the smallest number of empirical dimensions of psychopathology, its use in the period of this review appears to have undergone a relative decline. Recent factorial investigations (148, 159, 195–197) have for the most part been limited to a segment, rather than the entire range, of clinically interpreted psychopathology, and have had specific rather than exploratory objectives. Some of this work has already been referred to in the earlier sections of this chapter. A number of caveats (107, 138, 180) have been sounded against the misuses of factorial procedures in psychopathology, notably against inferring the independence of clinical entities or types proceeding from the factors isolated. For these purposes, a combination of factor analysis with a number of other techniques, notably cluster analyses and discriminant functions, have been more appropriately employed. Notable in this respect is the effort by Lorr and his associates (138, 139, 143, 145) who have pursued their program of identifying types of psychotic behavior, separately in populations of acute and chronic psychotic patients, on the basis of interview information and ward behavior. This procedure, involving a succession of steps and a variety of perspectives of observation, resulted in the reduction of the ten syndromes originally established to five: disorganized hyperactivity, schizophrenic disorganization, paranoid process, anxious depression, and hostile paranoia (143). Lorr, Klett & Cave explicitly noted the correspondence of this fivefold breakdown with some of the components of the scheme of classification by Phillips presented earlier.

Another group of approaches is anchored in the realization by a number of investigators of the differences in the prognostic and diagnostic values of symptoms. Such observations have been made, for example, by Lac-

quaniti (120) in a study of symptoms as well as diagnostic syndromes in an industrial population of Northern Italy. He found that some manifestations traditionally thought to be indicative of neurosis, e.g. nail-biting and sleep disturbances, were in fact of no differentiating value. The symptoms that proved to be encountered with different frequency in neurotic and non-neurotic groups were in the form of weakness, subjective discomfort, and emotional instability. Generally, however, it was the number and not the kind of symptoms that set apart neurotics from their unaffected peers.

In a more formal fashion, Nathan with co-workers (164-172, 247) has applied himself to the investigation of the specific relationships of symptom categories to the entire range of psychopathology of adults. The general theme of his results is the low degree of diagnostic specificity in such symptoms. The entity most successfully differentiated in this manner is psychosis, while supposedly neurotic symptoms occur at a higher rate of frequency within virtually the entire range of diagnosed psychopathology. Other factors being equal, differentiation on the basis of symptoms is best accomplished for broad psychiatric categories, e.g. psychosis, and is least effective in distinguishing specific syndromes, e.g. paranoid schizophrenia. The variety of symptoms that proved to be of greatest discriminatory value in this series of studies were in the domain of abnormal perceptual behavior and disordered consciousness.

Other approaches to the problems of the symptom-diagnosis relationship have made use of multiple discriminant analyses (106), multiple regression equations (252-253), and a number of configurational approaches (132). The German psychologist Lienert (132), for example, has addressed himself to the question of the minimal number and combination of symptoms that is sufficient for a fault-free diagnosis of depression. A number of specially devised configurational techniques has led him to the identification of five self-reported complaints which, occurring together, faultlessly predicted the diagnosis of depression. These symptoms concern inability to work or think, self-preoccupation, finding life painful to endure, and difficulty of getting up in the morning.

Going beyond diagnosis, Bucklew (24, 25) has focussed upon the link between personality patterns, as recorded in the case study record, and symptom expression. Reversing the traditional procedure, he has not only attempted to predict symptoms from the case record, but to trace retrospectively the case record characteristics from symptomatic manifestation. The findings that this work has yielded indicate broad patterns of continuity between the three general categories of neurotic personality, inadequate personality, and deviant personality, and their characteristic case record features.

From a different point of view, two recent studies by Phillips, Broverman & Zigler (187, 188) are also concerned with the relationship between the clinical phases of adaptation, symptom expression, and diagnosis. Their

results suggest that diagnostic classification in the broad divisions of the current system is effectively accomplished on the basis of social competence and symptom dominance information, the latter ordered two-dimensionally according to role and sphere. These findings are unexpectedly complemented by the reports of work by an Austrian research team whose aim was to predict post-hospital adaptation from multiple-regression equations based on symptoms. It is worth noting that the symptom patterns that proved predictive of a good adjustment were similar to those found to be associated with high social competence in this country (252, 253).

Thus the makings of a pattern of findings appear to be emerging. Symptoms are not directly translatable into diagnoses, although to a limited degree they have some suggestive diagnostic significance. The assignment of diagnosis appears to be a matter of assessing the individual's entire pattern of adaptation in the light of his clinical disturbance. The findings of Gauron & Dickinson (60) on the nature of information actually used by diagnosticians in arriving at their judgments bear out this impression; the clinician attends to items of social history as well as symptoms in arriving at his determination. The results reviewed also suggest that symptoms pooled or integrated convey more information than discrete manifestation; the section that follows is implicitly based on this recognition.

Scales.—The effort to construct and apply scales that yield dimensional rather than categorical diagnostic judgments has been vigorous and characteristic during the period of this inquiry. It is impossible to list, let alone to describe, all the instruments constructed or employed for this purpose. We will limit ourselves at this point to indicating their differentiating features which pertain to the person of the rater (clinician, nurse, nonprofessional rater, patient himself), the source of the observation (interview, word-observation, self-report), the balance of inference and evidence, the range of content (from all of psychopathology to one of its component dimensions), the emphasis on pathological vs adaptive aspects of behavior, and last but not least, the completeness and thoroughness of the effort directed by ascertaining the reliability, validity, and factorial composition of the instrument. Measures exemplifying many permutations of the above characteristics are available. What remains to be done is to describe at least some of the characteristic aspects of the work done with them.

One of the dimensions most thoroughly investigated by these means is depression (17, 42, 44, 79, 80, 93, 125, 195-197, 214, 272, 273, 275), possibly because of the ease with which it can be converted from a category into a dimensional characteristic suitable for comparison across individuals and situations. Other axes investigated by similar means include suspiciousness (43) and obsession-hysteria continuum (56). A test explicitly designed to diagnose schizophrenia has been devised by Hoffer & Osmond (105). The unique feature of this instrument is its construction on the basis of an ex-

PLICIT theory according to which schizophrenia is a biochemical disorder of adrenochrome metabolism with perceptual manifestations. The validation of this measure has gone beyond the traditional objective of diagnostic differentiation; explicitly, attempts have been made to correlate the total and partial scores on this test with the presence of a mauve spot in urine resulting from the alleged indicator of the biological disorder underlying schizophrenia. The results so far reviewed in two articles (41, 105) are quite promising even though the use of the test has not spread much beyond the settings in which it has been developed.

Eysenck's (46, 48) program as a facet of his dimensional, factorially based diagnosis (47) is much broader in scope, and over the past few years has been concerned with the factorial structure of self-report statements of normal populations. This research has succeeded in identifying neuroticism and psychoticism factors closely corresponding to those extracted from Eysenck's earlier factor analyses of clinical populations. The results of this work justify the expectation that neuroticism and psychoticism are variables comparable across unimpaired and clinically incapacitated pools of subjects.

A number of measures based on symptoms, observer ratings, or self-reported feelings and experiences (3, 10, 26, 62, 81-84, 104, 111, 158, 179, 184, 186, 189, 191, 195, 227-229, 256, 257, 264, 265) have sought to encompass much of the full range of psychopathology; some of them have been used simultaneously to establish the components of psychopathological experience. Several of these measures have been objects of serious and concerted research programs and have yielded findings that go beyond mere comparable scores. Rather, as exemplified by Lorr's (138) large-scale factorial investigations, the results obtained are useful to identify the major components that make up psychotic behavior.

While the studies described aim to order the multidimensional manifestations of psychopathology empirically, Meldman (e.g. 155) has attempted on a theoretical basis to reduce the manifestations of psychopathology to the continuum from an excess to deficit of attention. The actual scoring of Meldman's attentional profile for psychopathological states and individuals requires separate scores for manifestations of several kinds of attentional fluctuations. His method is heavily dependent on observations of the patient's cognitive and perceptual functioning and may fairly be described as bypassing gross observation of gross symptomatology in favor of its more basic organismic substrate.

In contrast to this work, all of which has been concerned with the clinically pathognomonic rather than the socially adaptive aspect of functioning, there is the growing tendency toward measuring and comparing the residues of strengths rather than the foci of disturbance, especially in chronic, long-term patients. Instruments such as the Social Dysfunction Scale (135), the Behavior Rating Scale (184), prognostic indices (231), and several versions of social competence measures (116, 186, 201, 218, 267) are expres-

sions of this increasing trend. Greater attention paid to these facets of behavior might answer the need felt by many workers of the learning theory or community orientations for instruments that are concrete, specific, and concerned with assets rather than liabilities.

The impact of work reported in this section goes beyond the sum of these diverse measures. Collectively, these scales and indices constitute an alternative to traditional diagnostic data gathering. Instead of classification of individuals into qualitative and global categories, multidimensional, quantitative comparison of dimensions of behavior is being attempted. In the process, the unity of etiology, description, and prognosis implicit in conventional diagnosis (271) is shattered; separate instruments are developed and applied to developmental, descriptive, and prognostic purposes. It is enough to consider the contributions to the recent conference report by Katz, Cole & Barton (101) in order to recognize the fractionation of diagnosis produced by the contemporary scaling effort; objectivity and specificity are attained at the expense of sacrificing global or configurational understanding, whether real or supposed.

DIAGNOSIS AS A RATIONAL DECISION AND AS A SOCIAL ACT

So far, we have said little about the contribution of the diagnostician to the task of classification. This issue has been investigated from two contrasting points of view. The practical challenges of computerizing diagnostic judgment have stimulated study in the rational, logical aspects of diagnostic categorization. Concurrently, another trend that has originated before, but continued during the period of this review, addresses itself to the social influences that affect the diagnostic process.

Diagnostic attribution as an instance of optimal information utilization.—

One of the characteristic developments of the last 4 years has been the progress made in formalizing diagnostic judgments by means of computers. Four threads of activity have been influential in this field. First, the practical task of standardizing information input and making it comparable across installations and subjects has led to the development of a number of data banks, useful for a variety of diagnostic purposes (e.g. 232, 243, 244). Second, several models have been developed that translate biographic and symptom information into diagnostic categories (21, 36, 65, 124, 126, 202, 212, 223, 225, 226, 258). Third, studies (44, 189, 190, 212, 224) have been conducted on the degree of concordance of clinically established diagnosis with its computer analog; generally, the results of these comparisons yield a high degree of agreement between these two kinds of diagnostic judgment and suggest that automatic, formalized diagnosis is a realistic possibility. Fourth and finally, there are investigators (e.g. 113, 114) who envisage a more ambitious eventuality—a computer-interviewee interaction replacing the dialogue between the interviewer and client. According to Kleinmuntz (114), the problems involved in simulating by computer and the open-ended,

flexible course of interview participation are technically soluble. Their application would open up a number of opportunities of observing extraction of diagnostic data without the benefit of a human relationship. Suffusing and transcending all of these specific objectives, there is a more general purpose to which computers have been put—that of implementing an optimally efficient succession of steps from the raw data of a person's life to its nomothetic representation in diagnosis. Retrospectively, Nathan (164) has applied such a model to the diagnosis of a number of classical cases in the history of abnormal psychology, e.g. Miss Beauchamp, Clifford Beers, Daniel Schreber, and Ellen West.

Computer methods, though influential, have not been the only ones to be applied to constructing formal models of classificatory activity. Bieri et al (15) integrate the principles of information theory with the task of reducing uncertainty inherent in cognitive categorization. At greater length, Stone (233–242) has devoted a recent series of studies to the formulation of psychophysical principles detectable in diagnostic comparison. Stone's findings permit him to place the diagnostic labels that psychiatrists and clinical psychologists employ on a prothetic continuum for such variables as prognosis, length of hospital stay, weight of constitutional factors, susceptibility to external stress. His conclusions emphasize the lawful and regular relationships among the principal diagnostic categorizations on each of the above clinically relevant continua. It should be added, however, that Stone's original and thorough work pertains to the clinicians' stereotypes of these disorders rather than to their real-life exemplars. It remains to be seen if the psychophysical relationships established survive their application to the comparison on the same dimensions of living individuals correspondingly diagnosed.

On a more general plane, Newbrough (173) has proposed, though not yet implemented, the application of psychophysical principles to the judgment of perceived behavioral discrepancies implicit in diagnostic categorization. Newbrough's scheme rests upon scaling the three pivotal variables that result in diagnostic designation: attaching verbal labels to acts or individuals, the degree of interpersonal distance in which this activity occurs, and the amount of tolerance in the given social setting for the behavior that is being observed and labeled.

Diagnostic decisions in a social context.—The study of the clinician's cognitive activity as a special case of forming judgment about people antedates the period of this review by at least 10 years. Nonetheless, we can record a particular flowering of conceptualization and investigation of such judgments from a variety of perspectives. Bieri et al (15) in this country and Ranabauer (194) in Germany have made notable contributions toward integrating the concepts and findings from the experimental study of cognition and perception of other people. They point to the opportunity for an-

choring effects to arise, for halo effects to obstruct the perception of diagnostically significant detail, and stress the value of diagnostic naming as a device at the service of reduction of dissonance and elimination of uncertainty. Ranabauer, in particular, points to the emotional overinvestment in diagnostic labels that results from the role that these labels play in reducing cognitive strain and camouflaging lack of information. In this she comes close to arriving at the conclusions reached by radically different routes by Sarbin (209-211) and Szasz (245) and recapitulated early in this review.

The empirical research concerning the clinician's diagnostic activity bears less on the operation of these cognitive mechanisms in process than it does on the products of several gross characteristics of the milieu, the clinician, and the diagnostic scheme.

Among these the influence of the client's socioeconomic class in facilitating the attribution of some, and impeding the application of other, nosological designations is particularly well documented (e.g. 69, 154, 217, 220). Some information is also available on the role of cultural, ethnic, or racial factors (e.g. 73, 102, 103, 176). Nonetheless, the findings converge in suggesting social distance as a mediating variable. Across socioeconomic or other subcultural lines, the middle-class diagnostician is prone to assign categories of severe psychology and to note fewer symptoms in the process.

The effects of experience on diagnostic attribution have also been investigated (102, 166, 247). Contrary to what one would expect on common-sense grounds, the correlation between years of practice and diagnostic activity is not altogether positive. In one study (247) it has been found that, in a simulated diagnosis situation, the experienced clinicians were selectively prone to the distorting effects of a faulty prestige suggestion of psychosis. Nathan (166) too has noted discrepancies in the diagnostic sensitivity of groups of psychiatrists differing in experience; specifically, the more experienced workers were more sensitive in diagnosing organicity. The less experienced ones were more efficient in detecting the more subtle perceptual deficits in functional psychotics. It would seem that sensitivities as well as stereotypes are built up in the daily commerce with disturbed behavior; the latter in particular might reflect the fluctuations of emphases in clinical training over the years.

The different diagnostic roles and experiences of the several clinical professions involved in diagnosis and observation, e.g. psychiatrists and psychologists, also result in characteristic patterns of inclusions and omissions in observational accounts of hospitalized patients' characteristics. In a study already mentioned (247), psychologists have been found to differ from psychiatrists in responding less to a prestige suggestion and in exhibiting a lower degree of "psychopathology bias."

Another variety of the distorting effects of experience is that of one's theoretical predelection and commitment; as a by-product of his research on depression, Kendell (108) has recorded a high degree of correspondence

between the belief of the unitary versus dual nature of depression and the diagnosis of cases that conform to the pattern in question.

More generally, the diagnostic system in use, whether American (153), foreign (263), or international (95), guides as well as misleads the users of a given system. A large share of diagnostic errors are thought to be traceable to this cause. Empirical studies have identified some portions of the diagnostic spectrum, too finely divided for effective diagnostic differentiation, and others, excessively broad in their vagueness (263). In reference to the revised diagnostic manual, the point has been made concerning the difficulty of empirically separating simple schizophrenia from schizoid personality.

Remarkably, findings become fragmentary as we turn to another potentially important parameter, that of the clinician's personality. We have found little information in the literature that deals directly with the influence of these variables. Two studies (61, 74) reported on the different styles of arriving at diagnosis, from diagnosis via exclusion to an intuitive assignment to a diagnostic category. While these stylistic features were readily detectable, the clinicians observed were not rigid or consistent in employing any one style from patient to patient. An as yet unspecified interaction phenomenon is suggested by these findings. The question remains as to what encounter of patient and observer characteristics results in what particular kinds of cognitive activity.

DIAGNOSIS ACROSS SPACE AND TIME

The points made in the preceding section apply to the two themes that remain to be discussed: the cultural and historical variations in both the diagnoses applied and the disturbed behaviors manifested.

Turning to culture first, there has appeared a growing number of demonstrations that the actual symptom manifestations of gross clinical psychopathology differ across cultural or ethnic lines (162, 163, 211, 219, 222), even when the groups compared are matched in diagnosis and relevant biographical characteristics (35, 57). The conclusion would seem warranted that identically diagnosed conditions in psychiatric settings are expressed through different patterns of symptomatology which, moreover, seem to be linked to the nature of the modal, unimpaired patterns of adaptation characteristics of the milieus compared.

In the light of data presented earlier, it would be idle to assign all of this variation to the patient and none to the diagnostician. To elaborate: while engaging in diagnostic classification, the clinician acts as both expert and agent of his cultural milieu. A number of studies (91, 206, 207) comparing the diagnostic judgments by British and American psychiatrists of patients viewed on the screen from the two countries have brought to the fore differences in readiness to diagnose affective and schizophrenic disorders, British observers being partial to the former, and Americans to the latter diagnosis. Several additional differences have come to light: British clini-

cians are more reliable on the diagnostic judgments than are Americans, yet Americans note a larger number of symptoms per patient. In another comparison involving psychiatrists in Mexico and in the United States (49), the ratings of severity of several symptoms have been found to differ in these two countries.

These and other more impressionistic findings concerning the diagnostician's contribution to divergence of diagnostic judgment have sparked an international effort under the aegis of the World Health Organization to standardize the extraction of diagnostic clues across national lines. Workshops and seminars have been conducted to pinpoint the sources of these diagnostic discrepancies and to take steps toward correcting them. These procedures have succeeded in reducing, but not in eliminating, differences in the application of diagnostic categories (134, 221). Other observations that have emerged from these diagnostic exercises concern the greater concordance in the diagnosis of severe dysfunction, e.g. schizophrenia, than of milder varieties of the disorder. In contrast to the methods described, based on personal meeting and exchange of observations, Pichot (189, 190) and Engelsmann et al (45) have approached the task of increasing diagnostic comparability by establishing the symptom bases for the rather different nosologies in use in the United States and in several European countries. This effort has resulted in the empirically based translations from one nomenclature into the other, as well as a residue of categories for which no counterpart could be found in some countries.

An even thornier issue divides the observers concerned with the worldwide comparability or incidence of psychopathological entities. While the above comparisons have been performed mostly in settings characterized by greater similarity than difference, the issue of worldwide incidence of major psychopathological disorders is particularly germane to students of non-Western, traditional, technologically simple societies. The majority opinion in the field (e.g. 4, 162, 208, 268) holds that diagnostic categories are universal in representation, though different in distribution. Several recent formulations (e.g. 39, 103, 127, 175, 192, 259), however, have come close to advocating the construction of culture-specific classification systems. The arena of this controversy is the phenomenon of culture-specific syndromes which to some (e.g. 268) represent disorders different in appearance, though identical in substance, to the diagnostic categories encountered in other settings. To others (e.g. 259), these disturbances are embedded in their own cultural context and can be meaningfully expressed through explanatory constructs, e.g. "ego strength" rather than through the assignment to diagnostic categories.

Other avenues of resolving this problem would be through the application of factor and cluster analyses to establish the universality or uniqueness of the empirical dimensions and types of maladaptation. Such research has been performed (31, 115, 140-142) and with notable exceptions (e.g.

31) has strengthened the case for the similarity of the statistically established components of disorder. These results are compatible with those of investigators who have reported intercultural commonalities in relationship between such antecedents of psychopathology as stress (33) and social competence (34) and their clinical manifestations.

Many of the observations recorded so far apply to the yield of the much smaller number of studies concerned with historical changes. Häfner et al (78) have traced the diagnostic practices of two university psychiatric clinics in Germany over 60 years and concluded that the theoretical orientation of the director was a more potent determinant of diagnostic designations than were wars, political disruptions, or economic upheavals. Some diagnoses, e.g. psychotic depression and schizophrenia, were more affected by these trends than others, e.g. mania. The Swiss psychiatrist, Schneider (215), has noted fluctuations in diagnostic practices during his own 20-year directorship and, with admirable candor, has attributed some of these to his own change in outlook. In Africa, Prince (193) has concerned himself with the sudden upsurge of reported depression in the post-independence era of the new African states. He identified four cumulative or competing sources of his phenomenon in the form of the prestige of depressive disorders as an index of sensitivity and sophistication, the shift of loci of observation from hospitals to communities, the broadening of the diagnostic category of depression, and its actual increase under the impact of westernization.

Thus not all of the change in descriptive information across time would lend itself to explanation in terms of diagnostic trends and fashion. One of us (184) has reviewed the evidence that points to more substantive changes in psychopathological manifestations rather than its reflection through the clinician's records. More recently, the work by Ödegård (174) in Norway, Varga & Nyiro (256) in Hungary, and Lenz in Austria (129) suggests changes in the symptom pictures, less amenable to differential elicitation and more closely related to the changes in social milieu, e.g. the decline of self-accusatory delusions, the increase of depression, and the shift from visual to auditory hallucinations. As in the case of cultural variations, it would seem that the subject and the observed both contribute to the reflection of their times through changes in maladaptive behaviors; we know of no research that fully separates these two sources of variance.

CONCLUSIONS

Perhaps the most general conclusion to be drawn from the work of the 4 years reviewed is that psychodiagnostic classification has moved far beyond the assignment of individuals to qualitative and mutually exclusive categories. A dimensional, multipurpose, pragmatic search for new ways in diagnosis stands in stark contrast to the historically inherited classificatory grid of global descriptions. Some of the categories of the earlier classification show convertibility into the newer quantitative concepts; other components

of nosology have, in effect, been bypassed by current conceptualization and research. In any case, the field of classification is no longer coextensive with the assignment of designations from the traditional taxonomic scheme.

As diagnostic activity moves into new directions, there appears to be a number of different and even contradictory trends. On the one hand, an increasing body of empirical findings and theoretical formulations argues against the absolute and unconditional nature of diagnostic categories and judgments. From an emphasis on the platonic ideal in diagnosis, we are moving to a consideration of the cognitive, perceptual, and interpersonal processes involving the diagnostician and the person diagnosed. On the other hand, a major share of diagnostic research is given over to the development, refinement, and application of procedures based on factor analysis, psychophysics, general systems theory, and cybernetics. All of these trends are predicted on the assumption of an inherently objective, albeit as yet imperfectly developed character of diagnostic categorization. To paraphrase Eiduson (37), diagnostic decision-making rests on the integration of two kinds of information: the former descriptive of the client's characteristics, the latter deeply embedded in the multilateral transactions between the clinician, the client, and their respective social milieus.

Another clash of polarities occurs in relation to the choice of units of psychopathological comparison. A characteristic feature of the last 4 years has been an emphasis on symptom, at the expense of syndrome, as the fundamental building block of diagnostic structures. Yet this trend has co-existed with and has even been informed by the recognition of the inextricable link between premorbid and clinical phases of an individual's life. This simultaneous pull in the direction of atoms and configurations has brought about tensions, ambiguities, and confusions; the issues revolving around the continuous versus alien character of clinically defined maladaptation remain vital subjects to be pursued conceptually and empirically.

One of the characteristic concerns of the present period has been the recognition of the worldwide problem of diagnostic classification. Cultures are increasingly being regarded as testing grounds for the empirical study of variable and constant features of maladaptation. Effort is simultaneously being directed toward establishing the comparability and translatability of diagnostic judgments. Again, two contrasting orientations are clearly in evidence: an emphasis on cultural plasticity of maladaptation is opposed to the view of worldwide hard core of pathology upon which subsidiary cultural accretions are superimposed.

Finally, the years under review have been marked by the introduction and testing of a variety of nondisease models applied to construing and categorizing disturbed behavior. Yet at the same time, this has also been the period during which the traditional construct of mental illness has demonstrated its remarkable viability and persistence. The endurance and reassertion of the construct of illness in the face of apparently cogent and incisive

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BEHAVIOR THERAPY¹

172

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In recent years most authors start their chapters in the *Annual Review of Psychology* with a declaration that the task of reviewing their particular field is overwhelming because of the literature explosion. We can do no less, especially in view of the fact that this is the first *Annual Review* chapter on behavior therapy. In preparing the material for this chapter we have compiled a bibliography of over 4000 items, most of which should be included in any comprehensive review of this field. Needless to say, space limitations in this volume and the finiteness of the author's life preclude full justice being done to all.

Rather than selecting a limited portion of this literature to review intensively, the goal of this chapter is to present a broad overview of the field. In fact, the major problem facing the reviewer is to define the parameters of the field of behavior therapy in such a way that it can be labeled and differentiated from other approaches to working with human problem behavior. Therefore we have sampled from a wide content area of publications so as to enable us to arrive at a comprehensive picture of current behavior therapy and the context in which it has developed.

Although this is the first chapter to be devoted wholly to the field of behavior therapy, several recent chapters in the *Annual Review of Psychology* on *psychotherapy* have included references to or sections on behavior therapy, or procedures now included within this rubric (68, 82, 238, 347, 383). As early as 1951, Hathaway (142) made a cogent argument for the importance of relating psychotherapy to *learning theory*. The following volume (287) included a section on *learning and psychotherapy* which was primarily a review of the book by Dollard and Miller (83). Subsequent authors (242, 304, 310, 318, 381) have devoted portions of their chapters to noting the "promise" in learning theory and conditioning approaches to psychotherapy and especially "the exciting possibilities of Skinner's methods" (162, p. 305) in their application to the behavior of psychotics.² Despite the brief mention

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² Gendlin and Rychlak, in their review in Volume 21, point out that by 1968 "it seemed clear that the behavior therapies were again the dominant preoccupation of the psychotherapeutic professions, and (as in the last 2 or 3 years) these techniques are now clearly the primary stimulus for publications in our area" (*Ann. Rev. Psychol.* 21:155).

throughout the years to these studies, there has been no attempt to bring together the field of behavior therapy as anything else but a variation of traditional psychotherapy.

Although emphasis in this review will be on publications appearing in the latter part of the 1960s, research in behavior therapy coincides with the whole decade of the 1960s.

DEFINITIONS AND PARAMETERS OF BEHAVIOR THERAPY

As far as can be ascertained, the first use of the term *behavior therapy* in the literature was in a 1953 status report by Lindsley, Skinner & Solomon (223), referring to their application of operant conditioning (of a plunger pulling response) research with psychotic patients. Lindsley (personal communication) suggested the term to Skinner, based on its simplicity and linkage to other treatment procedures. Independently of this early usage, and of each other, Lazarus (208) used the term to refer to Wolpe's application of reciprocal inhibition techniques to neurotic patients, and Eysenck (92) used the term to refer to the application of what he termed "modern learning theory" to the behavior of neurotic patients based in large part on the procedures of a group of investigators then working at the Maudsley Hospital in London.

The term *behavior modification* has also been used: interchangeably with behavior therapy, as a more generic term than behavior therapy, or to refer solely to the application of operant conditioning to distinguish it from the behavior therapy derived from the work of Wolpe (386). Homme et al (156) describe another related term, *behavioral engineering*, as a combination of the two technologies of *contingency management* and of *stimulus control*. An attempt at distinction between the terms behavior therapy and behavior modification is made by Keehn & Webster (180), who argue that behavior therapy may be placed within a traditional S-O-R paradigm because the aim is to modify the state of the individual alone, whereas behavior modification may be located in a newer stimulus-response-reinforcement paradigm which aims to alter the individual's *transactions* with his environment. Actually, the latter view subsumes the former, and hence both may be considered as belonging within behavior therapy.

It is especially crucial to determine the scope and limitation of the field of behavior therapy since too comprehensive a view (e.g. equating it with all of psychology) renders it meaningless, and too narrow a view (e.g. equating it with a specific technique) will render it useless. As one plods through the vast literature of behavior therapy, one is reminded of the parable of the blind men who described the elephant solely on the basis of their feeling the animal. They gave excellent accounts of tusks, legs, trunk, ears, but no version of a complete elephant. Our task in this chapter is to present a picture of the elephant and to demonstrate how he differs from other animals, for it is clear, to this reviewer at least, that the elephant of behavior therapy does indeed exist and that he can be discriminated from other creatures of the jungle.

One influential group of investigators has consistently defined behavior therapy in terms of learning theory. Wolpe (389) offers as his most recent definition: "behavior therapy, or conditioning therapy, is the use of experimentally established principles of learning for the purpose of changing unadaptive behavior. Unadaptive habits are weakened and eliminated; adaptive habits are initiated and strengthened" [p. VII].

A more encompassing framework comes from those who view behavior therapy in the broader context of social learning or behavior influence. Ullmann & Krasner (361) describe behavior therapy as "treatment deducible from the sociopsychological model that aims to alter a person's behavior directly through application of general psychological principles" [p. 244]. This is contrasted with "evocative psychotherapy" which is "treatment deducible from a medical or psychoanalytic model that aims to alter a person's behavior indirectly by first altering intrapsychic organizations." Another similar conceptualization is given by Patterson (263):

It seems to me that future trends will of necessity involve a greater reliance upon principles available from social learning. The term *social learning* as used here refers to the loosely organized body of literature dealing with changes in learning, or performance, which occur as a function of contingencies which characterize social interaction. . . . Many of the mechanisms which have been described for bringing about these changes have been based upon principles from social psychology rather than learning theory; these would include such processes as persuasion, conformity, and modeling [p. 342].

Heller & Marlatt (144) have argued that "More than any other group, the behavior therapists have recognized the desirability of deriving their clinical procedures from a more basic psychology of behavior change" [p. 572].

Kanfer & Phillips (172) classify four types of behavior therapy: "interactive therapy" (methods requiring extended series of personal interviews using the therapists' verbal behavior to catalyze changes in the patient), "instigation therapy" (using suggestions and tasks to teach the patient to become his own therapist), "replication therapy" (changing behavior by replicating a critical segment of the patient's life within the therapy setting), "intervention therapy" (disruption by the therapist of narrow response classes as they appear in the patient's interactions with his natural environment). They call for

establishing a well integrated framework from which a practitioner can derive new techniques with clearly stated rationales with predictable effects and with well defined criteria and methods for examining their efficacy . . . A consistent behavioristic view requires an understanding of the entire range of psychological principles which can be brought to bear on the problem of an individual patient, from his presentation to his discharge [p. 418].

Bandura's (25) social learning conceptualization of behavior therapy places major emphasis on controlled laboratory studies of efficacy: "By requiring clear specification of treatment conditions and objective assessment of outcomes, the social learning approach contains a self-corrective feature that distinguishes it from change enterprises in which interventions

remain ill-defined and their psychological effects are seldom objectively evaluated" [p. v].

Krasner (189) conceptualizes behavior therapy as comprising a series of specific techniques derived from the experimental laboratory, usually based on learning theory and applied in a *social influence* situation. Behavior therapy thus involves a technology built upon a *base* of behavior influence. What Lazarus (209) calls the "non-specific" factors, which others call "placebo effect," involves very specific social psychological variables such as expectancy, prestige, demand characteristics, set, experimenter bias, etc. The technology of behavior therapy becomes effective in modifying behavior only within the context of maximum social influence. It is important to emphasize that behavior therapy involves *both* the technology and its social influence base. Some of the behavior therapy investigators are clearly cognizant of this fact, but most ignore it. The behavior therapist is a compound social reinforcing and discriminative stimulus. He is the source of meaningful stimuli that alter, direct, and maintain another individual's behavior. Viewing the therapist as a "social reinforcement machine" (189) means that he is *deliberately* applying current behavioral principles within the relationship defined in social reinforcement terms to bring about change in another person's life situation (380).

The social influence base of behavior therapy involves procedures which could be defined in learning theory terms, e.g. enhancing the prestige or placebo value of the therapist means maximizing the social reinforcement value of the therapist. However, we are still at the early stage in which social psychology research is beginning to merge with the behavior therapy field. It is doing so in two ways. First, there is the approach of McGinnies (240), who reconceptualizes most of traditional social psychological studies such as small group behavior, attitude change, conformity, and leadership within the context of behavior principles, primarily Skinnerian. The second approach is typified by the application of specific social psychological theories or research to particular types of behavior therapy. For example, "attribution theory" (193) has generated considerable interest on the assumption that changes in behavior which the individual attributes to his own efforts will be more long lasting than changes attributed to external sources (81). Techniques then can be developed to find alternative nonemotional or artificial sources upon which to misattribute anxiety.

Goldstein (129) offers a rationale for studying *interpersonal attraction* as the major variable in the therapist-patient relationship. His investigations derive from social psychological research and belong within a behavior therapy framework. Thibaut & Kelley (353) view a person's attraction to another as a function of the extent to which the person achieves a reward-cost ratio in excess of a minimum level in his interactions with others. Based on these views and those of others, a social psychology of interpersonal attraction has developed which seeks to discover the behaviors and events which people find rewarding most of the time.

An important area of research for behavior therapy, thus far virtually untapped, is that of communication via nonverbal behavior. There is a considerable amount of research dealing with gesture, posture, communication accuracy, facial expressions, body cues, and eye contact. Space limitations prevent a full review of this field other than to indicate some recent general reviews (245) and some recent relevant research relating communication and social approval (86) and the use of body cues (87). Duncan (85) offers a review of nonverbal communication including voice quality, body motion, and use of personal space. Mehrabian (243) reviews the significance of posture and position in the communication of attitude and status relationships. The behavior therapists will eventually incorporate research findings on body posture and communication in their assessment of interpersonal influence and in training of new behaviors. Even at this point there is no adequate system of observing the specific cues which people use to signal each other in communication. Cueing is not innate but is learned social behavior since there are clear differences in cultural patterns of signaling.

Lazarus (212) advocates that behavior therapy not be limited to learning principles or learning theory. He refers to social influence processes and argues that Wolpe's methods are helpful but insufficient. "In essence, the broad-spectrum behavior therapist is free to employ *any* technique, derived from *any* system, without subscribing to any theoretical underpinnings which do not have the benefit of empirical support." Unfortunately this viewpoint could be used as an argument for a post hoc, haphazard use of principles rather than an a priori selection of procedures based on a theoretical rationale. The notion of behavior therapy offered in this chapter is not so broad as to include *anything* that works because it may then include fortuitous superstitious therapist behavior.

Following are some of the elements of the belief system common to behavior therapy: (a) the statement of concepts so that they can be tested experimentally; (b) the notion of "laboratory" as ranging from the animal mazes or shuttle boxes through basic human learning studies to hospitals, schoolrooms, homes, and the community; (c) research as treatment—and treatment as research; (d) an explicit strategy of therapy; (e) demonstration that the particular environmental manipulation was indeed responsible for producing the specified behavior change [this is the basis of those (e.g. 272, 395) who argue that behavior therapy is really an experimental approach in which each individual is an experiment in which $N=1$; even in desensitization the hierarchy is individualized by virtue of the individual's learning history]; (f) the goals of the modification procedure are usually determined by an initial functional analysis or assessment of the problem behaviors. In effect, what are the environmental determinants, maintainers, and consequences of current behavior, and what possible alternatives can be developed?

The unifying factor in behavior therapy is its basis in derivation from experimentally established procedures and principles. The specific experi-

mentation varies widely but has in common all of the attributes of scientific investigation including control of variables, presentation of data, replicability, and a probabilistic view of behavior.

Going beyond the delineation of the characteristics of behavior therapy is the drawing up of lists of ways in which it differs from traditional psychotherapy (92, 361). These lists range from those having a dozen or more clear differences to those de-emphasizing the differences or even calling for a bridging of the two approaches (335, 394). Whether it is possible, or even desirable, to combine the two approaches is of current controversy. Eysenck (95), for example, takes the position that bridging between behavior therapy and psychoanalysis is undesirable.

STREAMS OF DEVELOPMENT OF BEHAVIOR THERAPY

Behavior therapy can be broadly conceived of within the view of scientific paradigms in clash. Only if we can take a very broad perspective of the social implications of behavior therapy can we begin to see the revolutionary aspects of this approach to human behavior. If we accept Kuhn's (197) notion of changing paradigms, then in order to understand the current status of behavior therapy it is necessary to present the streams of development, the historical origins, out of which behavior therapy has developed, and the earlier paradigm to which it is in opposition. Burnham (55) applies Kuhn's (197) theory to the earlier period of Watson's contributions, and it is reasonable to view current behavior therapy as part of that same historic paradigm.

Katahn & Koplín (177) and Colby (68), among others, argue that the differences in the application of contemporary psychology and psychiatry can be represented by two competing paradigms. "The differences between them seem to focus primarily on the relative weight that internal information processing events will play in theoretical accidents of behavior, as contrasted with emphasis upon objective description of environmental events and secondarily on methodology" [177, p. 147]. There may be nothing in the laws of science that can help one decide on a logical ground which paradigm can be accepted, and many investigators are insufficiently aware of the influence on their own behavior of the scientific paradigm with which they are working.

There are at least 15 streams of development which have come together within the last part of the 1960s to form a distinctive approach to helping individuals with behavior socially labeled as deviant. These streams, of course, are not independent of each other; the merger of streams to form behavior therapy is far from complete (eventually these streams will be the genetic building blocks of the fully formed elephant).

1. Perhaps the most important general stream is that of *experimental psychology* and within it the concept of "behaviorism." A recent definition of behaviorism by Kantor (175) captures the spirit of this viewpoint.

What is behaviorism? . . . it is a renunciation of the doctrines of soul, mind, and consciousness. Positively expressed, behavioristic psychology is the study of the

behavior of organisms in interaction with their surroundings. The environment presumably consists of the innumerable concrete objects and conditions to which organisms must adapt themselves . . . Psychology in a sense lost its character as the seeker for the nature of the soul in order to confine itself to the study of the behavior of organisms. This meant an overt recognition that such behavior is the only material available for study and that whatever scientific flavor psychology has had throughout its quantitative, experimental, and mensurative periods was derived from the implicit appreciation of this fact [p. 357].

2. The field of *instrumental conditioning* tracing back to Thorndike and overwhelmingly influenced by the research and philosophical views of Skinner (326, 328–330, 333, 334) has represented the most influential stream in the development of behavior therapy as exemplified by the volume of research and application reported in the current literature. It is more than a chance phenomenon that Skinner's early work with Lindsley resulted in one of the three independent origins of the term behavior therapy described earlier.

3. A major influence on current behavior therapy has been the research, clinical work, and writings of the psychiatrist Wolpe (386–391), who in turn was influenced by the psychologists James Taylor and Leo Reyna. Wolpe introduced the technique of reciprocal inhibition, basing it in part on the classical conditioning research of Pavlov and Hull. This work engendered a considerable amount of research on the effects of desensitization with various "neurotic" behaviors.

4. A group of psychologists and psychiatrists, working within the framework of experimental psychology primarily influenced by Hullian learning concepts, practiced at the Maudsley Hospital in London under the general direction of Eysenck (93, 94). Their work represented the third independent development of the "behavior therapy" term (395).

5. There was a stream of investigators and practitioners in the United States, frequently working in educational institutions, who were applying behavioral, conditioning, and learning concepts to various problem behaviors. Influenced by J. B. Watson, this stream can be traced through the works of Mary Cover Jones, Burnham, Dunlap, Mowrer (252), Hollingworth, Guthrie, and more recently Phillips (277) and Pascal (262). In effect, these investigators can be retroactively labeled as behavior therapists.

6. There has been a group of investigators attempting to interpret psychoanalysis in learning theory terms (Hullian). The most influential work has been the Dollard & Miller (83) book and the papers of Shoben and Shaw of that same period. Their influence has been almost nil in stimulating new clinical applications but has been important in making learning theory a respectable base for clinical work.

7. An important stream is that tracing directly to the applications of the research of Pavlov (22, 109). Treatment procedures in the Soviet Union and to a lesser extent in this country have been directly influenced by Pavlovian classical conditioning. In its purest form, according to Franks (110), such

therapy is primarily neurological and concerned with the speculative central nervous system processes of excitation and inhibition by means of conditioning. Thus Soviet colleagues call themselves psychophysicologists. These techniques are used to treat a variety of disorders including sex problems, alcoholism, and obsessions.

8. The *social psychology* laboratory has contributed a psychology of social influence which is being increasingly incorporated into a behavior therapy framework as represented in this chapter (54, 129, 130, 240, 244, 337). The corollary to the laboratory research has been the stream of social psychology and sociological theory which has emphasized the importance of social role learning originating in the work of G. H. Mead and extending to current interactionist theorists such as Parsons, Sarbin, and Homans.

9. The field of *developmental* and *child psychology* has offered us a stream of research exemplified by the contributions of Sears, Miller & Dollard, Bijou, Baer, Gewirtz, N. Ellis, and Stevenson. The current emphasis is on vicarious learning via modeling such as the research of Bandura (24).

10. Investigations of the parameters of the social influence process have brought within its framework a series of human interactions previously seen as unique or discrete phenomena. This has included research on such clinical and social phenomena as hypnosis, placebo effect, demand characteristics, experimenter bias, subject and patient expectancy, and the effects of non-verbal cues in interviews. Investigators in these areas (239, 261, 299, 300) take the "skeptical" position that they are dealing with aspects of a broad social influence process and not with a unique phenomenon.

11. It is now clear that there are available behavioral, social learning alternatives to the traditional disease model of psychopathology as developed in the mid-nineteenth century. Every one of the current books on behavior therapy presents such alternative behavioral models. More than any other point, this change in the conceptualization of the target behavior is the key to modern behavior therapy.

12. In tracing the streams of development of behavior therapy, it is of importance to include one negative stream. That is, the apparent failure of psychodynamic and psychoanalytic psychotherapies as indicated by outside critiques and internal dissatisfaction. It is not a question of denigrating an opposing point of view but rather of becoming aware of the broader paradigm clashes that are involved. Behavior therapy grew, in large part, out of dissatisfaction with traditional psychotherapy techniques. It is in the sharp drift away from these procedures that we can view the development of alternative forms of treatment in the 1960s as a true scientific revolution. As Kuhn (197) expresses it,

Because it demands large-scale paradigm destruction and major shifts in the problems and techniques of normal science, the emergence of new theories is generally preceded by a period of pronounced professional insecurity. As one might expect, that insecurity is generated by the persistent failure of the puzzles of normal science to come out as they should. Failure of existing rules is the prelude to a search for new ones [197, pp. 67-68].

13. The training of clinical psychologists has traditionally been within the so-called "Boulder model" which conceived of the professional role of the clinical psychologist both as a research scientist and as a professional person who *applies* that science. Behavior therapy opens up the possibility of finally being able to successfully achieve the spirit of this training, demonstrating that the researcher and the clinician are *not* separate but are integral parts of one role.

14. Cognizance must be taken of a stream of influence from psychiatrists who have stressed the importance of observation of behavior and human interactions such as Adolph Meyer, H. S. Sullivan, and those therapists who attempted environmental manipulation in ward milieu programs and in some community programs.

15. There is an increasingly important stream that can be labeled *utopian* in its emphasis on planning the social environment to elicit and maintain the best of man's behavior. It includes an ethical concern for the social implications of behavior control, as well as offering blueprints for a better life such as Skinner's *Walden Two* (327). This stream can of course be traced from Plato's *Republic* to the setting up of a token economy on a psychiatric ward or in a community setting.

These historical streams of development are now converging into the field of behavior therapy. Thus behavior therapy is more than a series of techniques or the application of learning theory; it is a broad conceptualization of human behavior. Unless we view behavior therapy in this context, its applications and implications will be irrelevant.

THE LITERATURE OF BEHAVIOR THERAPY

Within the decade of the sixties there has been a growing number of books on various aspects of behavior therapy. Initially these have been books which were collections of previously published papers or contributed original chapters or both (16, 93, 94, 109, 110, 161, 194-196, 217, 255, 280, 305, 339, 360, 363, 393). The quality of the contributions has varied considerably, but on the whole these books have given a realistic representation of the field.

Other relevant books during this period have focused on integrated overall views of behavior therapy or specific aspects of it (7, 22, 29, 35, 43, 101, 130, 133, 138, 173, 225, 229, 233, 275, 278, 279, 283, 286, 303, 306, 345, 352, 389, 392, 395). A series of books have appeared which have been intended both as reviews and manuals with specific suggestions for others to carry out behavior therapy procedures (13, 37, 152, 220, 265, 312). There have been books which, although not directly on behavior therapy, still included important chapters on behavior therapy (40, 108, 323). Other books have been useful in the presentation of the laboratory research basic to behavior therapy (30, 43, 60, 103, 157, 288, 291, 308, 323, 340, 366). There are books which have offered a behavioral approach to personality (231), to assessment (251), to sociology (54), and to the field of abnormal psychology (361).

Note should also be made of a series of early papers which called attention to behavior therapy and behavior change procedures as belonging

within the mainstream of psychology, particularly relating them to various aspects of learning theory (17, 23, 135, 165, 167, 188).

An evaluation of the status of behavior therapy, particularly in terms of its growth and influence, must include a mention of the journals in which have appeared research reports on behavior therapy. Early operant conditioning research reports appear in the *Journal of the Experimental Analysis of Behavior*. For a time during the mid-sixties, *Behaviour Research and Therapy*, edited by Eysenck and Rachman (281), and first appearing in 1963, was the prime publisher of articles on behavior therapy. In 1968 the *Journal of Applied Behavior Analysis* appeared to publish "reports of experimental research involving applications of the analyses of behavior to problems of social importance." The *Newsletter* of the Association for Advancement of the Behavioral Therapies, established in 1965 as an interdisciplinary group of professionals interested in behavior therapy, proved to be a useful informal source of information on research, trends, and training in behavior therapy and later developed into the journal, *Behavior Therapy*. Additional new journals that are appearing at the end of the decade are *Conditional Reflex*, a Pavlovian journal started in 1968, and *Behavior Therapy and Experimental Psychiatry*. By the end of the decade articles on behavior therapy were appearing in most of the journals on clinical and social psychology, child development, psychiatry, nursing, and education.

POSITIVE REINFORCEMENT

This section describes the techniques which involve the systematic presentation of positive consequences to influence behavior. This approach is derived primarily from the operant conditioning studies of Skinner. Reynolds (291) offers a useful introduction to operant methodology:

Operant conditioning is an experimental science of behavior. Strictly speaking, the term *operant conditioning* refers to a process in which the frequency of occurrence of a bit of behavior is modified by the consequences of the behavior. Over the years, however, operant conditioning has come to refer to an entire approach to psychological science. This approach is characterized in general by a deterministic and experimental analysis of behavior [291, p. 1].

Fuller (112) was probably the first investigator to present a report of the deliberate application of operant conditioning in a clinical setting by shaping arm movements with a warm sugar milk solution in an 18-year-old "vegetative idiot." Subsequent research was strongly influenced by the previously mentioned Lindsley-Skinner (223) studies at Metropolitan State Hospital.

The research procedures with adults and children have followed the same principles and utilized the same specific techniques such as token economy (developed originally for adults and then extended to work with children) or the training of key environmental figures (originally developed for the treatment of children and then extended to adults) (302). The studies most recently reported within this framework are characterized by greater ingenuity

(artistry) in techniques, a greater range of problem behaviors tackled, and greater sophistication of design (155, 160). We distinguish between those studies which demonstrate experimentally that the stimulus manipulations were responsible for the behavioral change produced and the studies which use operant techniques in a nonexperimental manner. These latter do not demonstrate that the reinforcement techniques were *functional* in causing the behavioral change.

The research studies based on operant conditioning place major emphasis on the use of *contingent* reinforcement to strengthen specific behavior. Generally the studies fail to emphasize the social influence base to which the specific operant training procedures are added. An exception are the studies of Agrad, Leitenberg, and their co-workers (2, 216, 260) which indicate that reinforcement is necessary for and *adds* to the social influencing effects of *instructions* thus linking their operant work to the social influence process.

Martin et al (237) found that a phase-contingent system of reinforcement produced more rapid and uniformly positive changes in behavior than a system of reinforcing specific responses individually with a group of delinquent adolescents. The latter contingency system produced arguing and accusations of unfairness. In order to insure adequate reinforcement of progress made by youngsters functioning at different levels, it was necessary to set different behavioral standards and criteria of achievements to obtain similar rewards.

Barrish et al (34) illustrated the ingenuity implicit in most of this applied work by devising the "good behavior" game. This involved the use of individual contingencies with group consequences. As a variation, Bushell et al (56) applied group contingencies in the classroom. Specific extrinsic incentives are vital during the early phases of behavioral change programs (141).

Addison & Homme (1) offer a reinforcing event (RE) menu to children from which they may choose what they would like to do at any given moment. Ayllon & Azrin (10) introduce the technique of reinforcer sampling in which patients are required to engage in a reinforcing event every time it is available.

Zeilberger, Sampen & Sloane (396) demonstrated that behavior can be modified more quickly the second time around in training a mother as therapist for her four and one-half year old boy. This is quite consistent with most *graphic* reports of the operant based reinforcement programs.

Sherman & Baer (322) make two important points about the extrapolation of animal experimentation to human application. First, too quick a success with a procedure may be self-defeating in that it becomes used before its function has adequately been established. Secondly, "these techniques, when applied to humans, often become modified; thus they are not even exact replications of the familiar animal laboratory techniques" [p. 217].

Baer, Wolf & Risley (19) make an astute and highly relevant comment that "generalization should be programmed rather than expected or la-

mented." This view represents an alternative to the usually futile approach, even among some behavior therapists, of changing a behavior in the laboratory or clinic and hoping that it will "carry over" to another situation such as the home. Bijou et al (44) review the methodology of experimental studies of young children in natural settings. The earlier studies of this group of investigators were of major influence in applying positive reinforcement procedures to children in various school settings (e.g. 384). Ferster & Simons (102) stress the use of *natural* reinforcers available in the environment (determined by careful observation) in shaping new behaviors of young children, in contrast with the more rigid dependence upon *food* derived from animal experimentation. Lovaas (227) presents an overview of the rationale and procedures of his highly influential series of research studies with childhood schizophrenics. Risley & Hart (294) report that nonverbal behavior in preschool children can be altered by reinforcing related verbal behavior.

Training of reinforcement dispensers.—Patterson (264) presents an overview of behavioral intervention procedures in the home and in the classroom. He offers a key element in behavior therapy by viewing intervention as "attempting to *modify* the *dispensers* which provide" reinforcement contingencies in the social environment. Most of the studies which use positive reinforcement also involve training individuals in the techniques of reinforcement, including how to observe and how to reinforce behavior (46, 136, 266). Such training is done individually and in groups with the aid of training devices such as films, closed circuit TV (41), and bug-in-the-ear (186), in the laboratory, in the home, and in the schoolroom. The training itself is done by psychologists, by technicians, by parents, and by teachers (353).

Patterson's (264) conclusions after his thorough review of family intervention procedures within the context of overall research utilizing positive reinforcement is that

this body of research represents one of the few developments within psychology in which the data have the dual characteristics of being replicable and meaningful. . . . At the present time the greatest immediate area of weakness lies in the inadequate follow-up data for studies carried out in the normal classroom and inadequate generalization data for studies carried out within the token culture classroom. . . . By contrast to the classroom intervention procedures, the technology for family intervention is very primitive. In addition to the limited range of problems and families investigated thus far, there are almost no data available which demonstrates persistence of effects. While the existing technique *could* be applied in practical settings, it is almost certain to be the case that each investigator would find himself generating some innovation to meet the exigencies provided by each family. This then implies that for at least a few years, most of the applications are likely to be made by trained behavior modifiers who are testing and developing a new technology.

Patterson, Shaw & Ebner (268) describe the use of peer-reinforcers and a "work box" for training attending behavior. They illustrated how the operant training strategy works despite the fact that the child whose be-

behavior was being modified was aware of what was going on and even protested that "this won't work." Wahler (369) successfully modified extreme oppositional behavior in children by altering their parents' reinforcement practices. In the initial part of the program parents ignored their children's resistance to requests and rewarded cooperative behavior with approval. This procedure proved to be relatively ineffective. The next procedure involved a reinforcement system which combined parental approval with tokens exchangeable for toys. This more sophisticated approach produced dramatic increases in cooperativeness. Then the tokens were gradually withdrawn, and cooperative behavior was maintained by social approval alone.

Bernal (41) reports a procedure for training parents in child management. She used operant conditioning and behavioral feedback via television. Cantrell et al (58) use the technique of contingency contracting. The authors prepared written contracts which were administered by natural contingency managers (teachers, parents) who kept daily records of behaviors and contingencies. Evans & Oswalt (90) report on the use of peers in accelerating academic progress. Fifth graders have been trained to shape the behavior of first grade students (351).

In addition to the specific token programs, there are reports of several programs which involved the training of the teachers in general reinforcement procedures (137, 158, 301, 364). Wahler & Erickson (370) describe a successful reinforcement-based community therapy program carried out by trained nonprofessional volunteer workers in the Appalachia region. Nurses have been a frequent recipient of reinforcement training procedures. Madsen, Becker & Thomas (232) report that the behavior of elementary school teachers can be varied systematically to determine the elements of classroom control.

Verbal conditioning.—Research in the area of verbal conditioning has been one of the major linkages between the experimental laboratory and clinical applications. The importance of this aspect of behavior therapy lies in the nature of the behavior involved, namely human verbalization. The first verbal conditioning study (134) to attract widespread attention was that of Greenspoon (published in 1955) in which he used a simple verbal response to influence the frequency of emission of the verbal response class of plural nouns. From such humble beginnings have grown reams of research, doctoral dissertations, controversy, and an approach to key issues of social influence. In fact, Dollard & Miller (83) seized upon the early reports of Greenspoon's findings to demonstrate their belief that changes in verbalization in psychotherapy were "automatic and unconscious." Whether this assessment was accurate or not (still a major controversy) Dollard & Miller anticipated the theoretical importance of verbal conditioning for clinical psychology.

In his most recent review of this field, Kanfer (169) argues that research on verbal conditioning has undergone four stages: (a) demonstration, (b)

re-evaluation, (c) application, (d) expansion. This is a useful way of summarizing these studies, particularly as they are related to behavior therapy research. The studies in the first stage *demonstrated* that verbal behavior could be brought under the control of environmental stimuli; verbal behavior followed the same principles as human motor behavior and that of animal behavior. The early verbal conditioning studies were similar to other early operant conditioning studies; they demonstrated that reinforcement *under certain conditions* can systematically influence verbal behavior. The second stage, that of *re-evaluation*, demonstrated that what was being dealt with was a far more *complex* phenomenon than at first was evidenced by a simple operant explanation. Responsivity to verbal conditioning was affected by variables such as social setting, previous experience with examiner, expectancy, variations in the meaning of reinforcing stimuli, and other interpersonal variables. In the third stage, that of *application*, operant conditioning was used to specifically change verbal behavior with a therapeutic intent e.g. Williams & Blanton (377) report a study in which verbal conditioning was used as a deliberate "therapeutic" technique and found to be as effective as traditional psychotherapeutic procedures. There have been other reports of a similar nature (132, 159). Gelfand & Singer (122) report a study in which verbal conditioning was used to influence the positiveness of the evaluation of photographs. The fourth, or *expansion*, stage of development involves those studies investigating theoretical issues related to the capability of human beings for self-regulation. These include processes such as vicarious learning (169), the role of awareness in learning (147, 191, 298, 367), self-reinforcement and self-control (196), and the associative relationship of words.

Behavior therapy involves focusing on the environmental cues which may serve as reinforcers (i.e. food, cigarettes, smiles, toys, tokens, head nods, "very good," etc.) as well as on what kind of verbal behavior may serve as a response class. Salzinger (307) reviews his many studies on defining the response class in verbal conditioning and offers several ways of describing the "complexities" involved in determining what a response class is.

Goldiamond & Dyrud (128) have explicitly extended the operant investigation of verbal behavior into the area of behavior analysis of psychotherapy. They make the same observation that many of the verbal conditioning experimenters have, namely, that there is a similarity between the operant conditioning strategy in research and that of the practitioner of psychotherapy.

Their observations are of major relevance theoretically and practically to behavior therapy.

We decided to observe therapy sessions further, to ascertain the relevant transactive units and procedures employed in the change [p. 64] . . . Much of the verbal conditioning research with nods and 'uh-huhs' may be beside the point when it assumes that such reinforcers change behavior therapeutically. They may merely keep the patient talking and maintain the behaviors of going through the implicit

program. The brunt of the therapy may be borne by the content of the transactions. We have discovered in our research that our 'uh-huhs' and nods, and many questions and statements as well, merely serve to maintain behavior. When reinforcements are given which shape behavior these are quite often noticeable and quite rare [p. 83].

In perspective, verbal conditioning studies are a research technique that developed as a combination of the operant conditioning approach and clinical interest in verbal behavior, hence within the behavior therapy framework. Many of the early investigators were interested in the process of psychotherapy, which during the early 1950s was primarily of the evocative model. Here at last it seemed as if operant conditioning offered a technique for setting up an *analog* of psychotherapy in a rigorously objective manner. It is clear now that verbal conditioning and psychotherapy are not the same identical process nor is one an analog of the other. However, some verbal conditioning does take place in evocative psychotherapy, and some of the relationship variables of the latter cannot and should not be eliminated from the former (118).

Almost every conceivable variable in the situation has been investigated in hundreds of studies, often with contradictory findings. The major uncontrolled variable has been the examiner: his expectancies, biases, and the interactions of his reinforcing characteristics with other variables of the situation. Studies investigating several variables at the same time have demonstrated complex interactional effects. It has been this very sensitivity of verbal conditioning to the many variables of human interaction which has emphasized its usefulness as a research device.

Token economy.—One of the techniques emerging from the early operant studies has been the use of tokens in place of primary reinforcers. Staats et al (341) were the first to utilize a back-up reinforcement system in a reading discrimination program. They used tokens exchangeable for a variety of edibles and toys. This meant that the therapist, or experimenter, was no longer dependent upon the momentary desirability of an object, e.g. M & Ms. Tokens opened up an almost limitless world of reinforcers.

The first report of a token program in a psychiatric hospital was that of Ayllon & Azrin (9, 11). This program evolved from previous techniques of Ayllon and his associates which applied the earlier Skinner-Lindsley principles of contingent reinforcement to shape desirable behavior. The early work of Ayllon & Michael (12) in training the psychiatric nurse as a behavioral engineer, and the use of satiation as an aversive procedure, opened the way for the introduction of the more encompassing token program. The Ayllon & Azrin token program functioned on a ward in a Midwestern state hospital with a population of long-term female patients. Atthowe & Krasner (8) report the successful use of a token economy program in a VA hospital in California with male patients averaging 58 years of age and median length of 24 years hospitalization. Schaefer & Martin (311) report the ef-

fectiveness of a token program in modifying a specific behavior, that of "apathy." The significance of this program lies in its reconceptualization of traditional patient withdrawal as "apathy," operationally specifying the specific behaviors that are included in this more general category, and the shaping of alternative, mutually exclusive behaviors which are prosocial. Since these early token programs, similar techniques have been extended to other hospital wards (224, 344), institutions for retardates (21), delinquents (317), the schoolroom, and the community.

Winkler (382) reports a series of research on token economy programs which extends this technique into an important new dimension, that of testing predictions from economic theory, thus studying the core of token programs. He investigated the complex relationships between the variables of prices, wages, and savings, and how they influence individual behavior. Token economies are found to operate according to principles similar to those which economists have found in national, money-based economies. Token economies not only look like "real" economies, they function like them, and research in this field has linked behavior therapy with social and "utopian" planning (192).

The comparison of one technique with another is a difficult undertaking at best because of the multiplicity of variables involved. In one of the few direct comparison studies, Marks, Sonada & Schalock (235) contrasted two groups of patients using token reinforcement procedures (improved behavior rewarded with poker chips exchangeable for meals) versus relationship therapy (each patient meeting daily with an individual therapist). The authors concluded that both therapies were effective in improving the behavior of chronic hospitalized patients, but the reinforcement therapy was more economical of staff time.

A series of investigators have applied the token economy technique to antisocial (52), delinquent (67, 104), and "character and behavior disorders" (69). The basic assumption in all such programs is that antisocial behavior is "acquired, maintained and modified by the same principles as other learned behavior" (52). Cohen's (67) analysis of alternatives to delinquency behavior resulted in the selection of academic work in the form of programmed instruction as the target behavior to be shaped by tokens. Cohen and his group then developed a special environment which included classrooms, study booths, control rooms, library, store, and lounge. Clements & McKee (66) used a variation of the token reinforcement procedures by combining contingency management with "performance contracts" in their work with 16 prison inmates. Such contracts are an important element in the development of self-management or self-control since the individual really sets his own behavioral goals.

Token economy in the classroom.—The use of reward itself in the classroom is of course not new, but the *systematic* use of contingent reinforcement by a teacher trained in the procedure does represent a basic change.

The principles behind the use of tokens in the classroom, contingency reinforcement of desirable alternative behaviors, is the same as in mental hospitals, but there are problems unique to the nature of the classroom.

O'Leary & Becker (258) introduced the first use of a token reinforcement program to control a large class ($N=17$) of emotionally disturbed children. The utilization of a token program resulted in a decrease of average disruptive behavior (talking, noise, pushing, and eating) from 76% in base period to 10% during the 2-month token period.

O'Leary et al (259) worked with seven children in the second grade and found that rules, structure, praise, and ignoring of the behavior were *not* effective in reducing disruptive behavior while a token program utilizing *peer competition* was effective in reducing disruptive behavior.

In an elaborate token program, Hewett, Taylor & Artuso (148) formed six classrooms of eight to eleven emotionally disturbed children. One class received tokens all year, one class none; the other four classes were split between receiving tokens and no reinforcement. The dependent measures were arithmetic and reading achievement and task attention. The experimental class receiving tokens significantly improved in performance when compared with the control classes. Wolf, Giles & Hall (384) describe a special remedial education program for 15 fifth and sixth-grade children in an urban poverty area. A trading stamp plan was used. Stanford Achievement Test Scores of children in the token program increased 1.5 years as against .8 years in a matched control group.

As other examples of variations of token programs designed to elicit desirable alternative behaviors to replace deviant or annoying classroom behaviors, there has been the use of money receipts with institutionalized female adolescent offenders (247), the contingent use of tokens based on performance on a news test (357), the contingent use of plastic washer tokens as tickets for special events (56), tokens to school dropouts in a Neighborhood Youth Corps who were "hired" to complete remedial workbook assignments (65), the use of contingent tokens in a remedial reading program (139), points as tokens, time out, and parental involvement with disruptive fourth to sixth graders (371), poker chips to increase "instruction following behavior" in retardates (397), and tokens extinguishing tantrum behavior (236).

Token economy in the community.—One of the most important recent developments has been the extension of the the token programs beyond the hospital and the schoolroom directly into the community. Miller (248) introduced the notion of freedom money, a token economy approach to organizing self-help activities among low income families. Henderson and his group at Spruce House (145) report a token program that bridges the gap between a residential institution and the community itself with adults who had seriously disturbed behaviors.

An example of a token program extended into the home with relatively

intact individuals is reported by Stuart (349), who worked with four separate couples who had sought help for marital problems. Labeling his approach "operant interpersonal," Stuart stressed the importance of constructing situations which could increase the intensity of interpersonal reinforcement. He trained his clients in the game-like procedure, called "prostitution," of setting up a token program in the home facilitating the exchange of items such as relevant talk and sexual intimacy.

It is clear that token economy programs are highly complex prototypes of behavior therapy and illustrate the social influence base of behavior therapy. The tokens per se serve several purposes, chief of which is to function as a vehicle (or excuse) for training of key environmental figures such as nurses, aides, parents, and teachers on *how to observe the behavior* of others and *how to use their own behavior* to reinforce desirable behavior of others. Kuypers, Becker & O'Leary (198) discuss the failure of a token program in the classroom because it did *not* involve the training of the teacher in behavior theory, nor was there a coupling of *social reinforcement* with the tokens, an essential element in token programs.

Use of apparatus.—Those using positive reinforcement procedures have developed a variety of apparatus to enhance their effectiveness. Schwitzgebel (316) critically surveys approximately 50 electromechanical devices in terms of five categories of function—monitoring and recording, stimulus transformation, neurological alteration, consequence scheduling, and teaching. On the positive side, such instrumentation should improve the reliability of behavior therapy procedures; on the negative side, many are of dubious value and tend to be gimmicks which detract from the interpersonal nature of behavior therapy. An ingenious use of "conjugately programed closed-circuit television" between a patient and a therapist is reported by Lindsley (222) and Nathan, Smith & Rossi (254).

DESENSITIZATION

In terms of sheer output of research, the desensitization branch of behavior therapy ranks just behind that of positive reinforcement. As a technique which involves the gradual substitution of a favorable (i.e. relaxation) response that is incompatible with an unfavorable (i.e. anxiety) response to a stimuli, desensitization has had many diverse clinical forebears. These include techniques labeled as deconditioning, reconditioning, associative inhibition, and autogenic training. However, it was Wolpe's (386) version of reciprocal inhibition which eventually stimulated the outburst of research and clinical application that has characterized the decade. In a recent description of reciprocal inhibition, Wolpe (388) summarizes his theory as follows:

A competing response is made to interfere with the response that one wishes to abolish. The competing (incompatible) response must therefore be elicited in the presence of the stimuli that would evoke the undesired response, and must be 'stronger' than the latter. The undesired response is then inhibited, and the

strength of its habit consequently diminished. Since the source of the inhibition is the competition of a second response, and since under other circumstances the situation could be reversed and the second response could be inhibited by the first, it is appropriate to describe the process of change as conditioned inhibition based upon reciprocal inhibition [pp. 234-35].

The term "systematic desensitization" has become widely used to designate the technique used to achieve the goal of response substitution. Wolpe (389) conceives of systematic desensitization as "the breaking down of neurotic anxiety-response habits in piecemeal fashion. A physiological state inhibitory of anxiety is induced in the patient who is then exposed to a weak anxiety-arousing stimulus. The exposure is repeated until the stimulus loses completely its ability to evoke anxiety" (p. 91). Based on the "conditioned inhibition" notions of Hull, Wolpe devised a series of animal experiments which resulted in the development of a technique of replacing an anxiety response by a relaxation response. Jacobson's (161) technique of progressive relaxation offered the possibility of a response which could be influenced by the therapist and which when developed would be incompatible with anxiety. Wolpe (386) paired muscular relaxation with visualized scenes and objects to which inappropriate anxiety responses had been made. Paul (269) described a relaxation procedure that can be taught in half an hour. The potentially threatening situations with which relaxation is paired range in a series or "hierarchy" moving from the least to the most threatening situation. Wolpe & Lazarus (392) and Wolpe (389) offer examples of hierarchies, as do many other recent investigators (211).

There have been a number of "explanations" of systematic desensitization which attribute change to "habituation" (200), to operant conditioning (215), to attribution theory (365), to interpersonal strategies, and to a Jungian analysis (375). As in each of the other techniques, the phenomenon seems far more complex than its originators have conceived (282, 284).

Evaluation of desensitization studies.—Paul (273, 274) offers a comprehensive and critical review of all available controlled studies of individual and group desensitization. Covered in detail are 75 papers (most published between 1959 and 1968) involving the application of systematic desensitization therapy to almost a thousand different clients by over 90 different therapists. Paul's conclusions are of importance because of the thoroughness and critical approach in his review.

While 55 of these papers were uncontrolled case reports or group studies without sufficient methodological controls to establish independent cause-effect relationships, 20 of the reports were controlled experiments, and 10 of the controlled experiments included designs which could potentially rule out intraclass confounding of therapist characteristics and treatment techniques. The findings were overwhelmingly positive, and for the first time in the history of psychological treatments, a specific therapeutic package reliably produced measurable benefits for clients across a broad range of distressing problems in which anxiety was of fundamental importance. 'Relapse' and 'symptom substitution' were notably

lacking, although the majority of authors were attuned to these problems. Investigations of equal quality and scope have not been carried out with other treatment techniques deemed appropriate for similar problems, and cross-study comparisons where control is absent have little meaning [274, pp. 158-59].

Of these studies only eight were sufficiently controlled to rule out confounding variables. All of these found evidence for the specific effectiveness of systematic desensitization. Of these only Paul's study (269, 270) adequately *compared* desensitization with traditional therapeutic procedures. The specific maladaptive behavior in this study was anxiety in a public speaking situation. Desensitization was compared with insight therapy, placebo, and a control group. The results indicated that desensitization was the most effective technique as compared with the other procedures.

Marks & Gelder (234) review approximately 35 "controlled studies of desensitization," and come to conclusions similar to those of Paul. Of these, 13 were controlled studies of desensitization in phobic disorders with volunteers, mainly students with isolated fears.

Nearly all investigations showed desensitization to produce more change in the treated fear than did the corresponding control treatments, which included relaxation, graduated exposure, flooding, visualizing non-phobic scenes, suggestion and hypnosis, insight psychotherapy, drug, placebo, and no treatment or a period on a waiting list. Only live modelling with guided exposure has produced superior results [p. 79].

Of equal interest is the Marks & Gelder review of four controlled studies (all by Marks, Gelder, and their colleagues, e.g. 120) in psychiatric patients with phobic disorders. They concluded that "desensitization yielded superior results to other treatments in patients who have circumscribed phobias without severe generalized anxiety but is not particularly useful in patients with high levels of free-floating anxiety" [p. 80]. An important contrast between the review by Marks & Gelder and that of Paul lies in criteria of what is a controlled study. Paul points out the absence of a real control group and the questionable reliability and validity of the ratings in the Marks & Gelder studies. Further, the relative inexperience of the therapist with the desensitization technique was part of a "complete confounding of therapist effects with treatment technique" [p. 101]. Paul's survey classifies the Marks & Gelder studies among the "uncontrolled case reports," hence downgrading their significance.

Variables in desensitization.—Investigations of desensitization have focused on some of the following variables which appear to be involved: therapist characteristics, client characteristics, duration of scenes and interval between scenes, number of hierarchies and order of presentations, methods of signaling anxiety, the generalizability of change, the interaction of systematic desensitization with other behavioral techniques, the presence or absence of the therapist during desensitization, the presence or absence of

relaxation training, and possible correlated physiological arousal. Only a few of the multitude of research studies can be cited (32, 72, 73, 119, 125).

Garlington & Cotler (116) compared desensitization plus relaxation against a no treatment group in modifying test anxiety. They found significant changes in test scores but not in behavior between the two groups. The treatment group decreased in test anxiety but received no better grades. (Test anxiety behavior has been very popular as a testing ground for desensitization theories because of its ready availability in colleges.) In contrast, the experimental desensitization group of Johnson & Sechrest (163) received better grades as against a relaxation only and no-treatment group of test anxious college students.

Bergin (39), in a case report, argues for integrating desensitization with traditional procedures involving warmth, empathy, and interpretation. These would be combined with each stage of the hierarchy development. Such mixing of models may be confusing to both the therapist and client.

One of the important but almost neglected therapist variables has been the amount of *experience* of the therapist in desensitization (213). If you conceptualize the technique solely as a conditioning experience for the client, then the therapist variables beyond a minimum of competency should be irrelevant. If on the other hand desensitization is a specific learning situation built upon a base of social influence, then the therapist variables are highly relevant. In a series of recent studies, the role of the *therapist* in desensitization has been investigated. Kahn & Baker (164) found a therapeutically directed desensitization to be as effective as a do-it-yourself variation with the therapist absent with 16 phobic patients. Donner & Guernsey (84) and Krapfl & Nawas (187) obtained similar results. In contrast, Ritter (296) found more effective desensitization with the therapist *present* when working with acrophobics.

Lang and his co-workers (203, 204, 206) have undertaken the most systematic exploration of the therapist variable in systematic desensitization. They have developed a programmatic apparatus known as DAD (Device for Automated Desensitization) which can conduct desensitization sessions by storing hierarchy and relaxation instructions on magnetic tape. When used with a client it automatically presents instructions in hypnosis and relaxation and the items of the prerecorded hierarchy in sequence. Based on early work with this apparatus, Lang (201, 202) concludes that "The fact that a programmed device can 'sit in' for the experimenter during a fundamental part of the therapy is the strongest kind of evidence that it is the specific program of instruction and not therapist or relationship variables that is the viable aspect of desensitization therapy" [p. 167].

The difficulty in the ability to identify and control the relevant variables is illustrated by one of several controversies between investigators. Davison (80) criticizes the study of Folkins et al (106, 107) on the basis of several errors in design. Folkins et al tested desensitization in a laboratory threat situation. The subjects received one of four training procedures before ex-

posure to a stressful film, desensitization, relaxation, "cognitive rehearsal," or no training. The results indicated that the separate components of desensitization, relaxation, and cognitive rehearsal were as effective as the complete desensitization program. However, Davison argued that the experiment was confounded by all subjects being instructed to "relax" while viewing the test stress film, and the subjects in the desensitization condition *not* being allowed to escape disturbing aversive images contrary to the principle of pairing of aversive scenes with relaxation, not with emotional upset. Folkins et al (106) defend their position by pointing to a "flaw" in Davison's (79) earlier study in possibly allowing some "desensitization" elements to confound a "relaxation only" condition. The important issue in this controversy, as it is in the entire behavior therapy field, is the ability of investigators to isolate the *specific* elements involved in their procedures (e.g. 70).

Implosive therapy.—One of the major approaches to desensitization is conceptualizing it within the framework of laboratory studies of human fear. The extensive research on fear reduction in animals has evolved into two directions. First has been systematic relaxation designed to gradually re-expose the individual to the fear situation. The opposite direction has evolved into implosive therapy. Stampfl & Levis (342, 343) have developed a treatment paradigm based on a simple extinction theory. Rather than the graded hierarchy presentation with relaxation, Stampfl presents only the items that would be on the top of the hierarchy, the most frightening scenes, hence maximal anxiety. With the repetition of these scenes it is assumed that the anxiety response will extinguish and that this will transfer from imagery to real life. From this work there has stemmed a growing and impressive series of research on implosive therapy (33, 150, 151, 378).

AVERSIVE PROCEDURES

Aversion therapy is the most controversial of the four major techniques of behavior therapy in terms of theory and ethical implications. Men have always used noxious stimuli to influence the behavior of other men, sometimes to coerce them, sometimes to treat them, sometimes one under the guise of the other. Within the context of behavior therapy there are two broad types of aversive methods, those derived from operant and those derived from classical learning theory.

Bandura (25) categorizes "aversive control" into the use of negative reinforcers, the use of aversive contingency systems, and the removal of positive reinforcers. Kanfer & Phillips (173) distinguish between punishment (an aversive stimulus contingent on a response expected to *decrease* its frequency) and escape and avoidance (an organism which is under noxious stimulation is reinforced in any act which *terminates* the stimulus). Rachman & Teasdale (286), in their analysis of the application of "aversive therapy" to behavior disorders, place major emphasis on the controversy

between the use of chemical or electric aversion techniques and lean toward the latter approach. Brush (50) reviews the experimental laboratory studies of aversive conditioning.

Azrin & Holz (13) review the *punishment* literature to which their research has contributed so heavily. An aversive stimulus suppresses responding when responding produces it, hence it is acting as a punisher. The same stimulus has an opposite effect on responding after being paired with a positive reinforcer. Thus a punisher could be used to maintain response by giving it discriminative or conditioned reinforcing properties. Solomon (336) deplored the neglect of aversive stimulation in research because of the legend (created in part by Skinner) that punishment is ineffective, produces side effects, and is temporary (response inhibition).

There has been relatively little work on the variables of stimulus *intensity* in avoidance learning in humans. Bucher & Lovaas (51) found that such noncontingent stimuli as a loud "no" or slapping with autistic children reduced smiling and babbling but increased echolalic speech and looking at the adult's face, whereas pleasant stimuli had the opposite effects. The suppressive effects of the noxious stimuli were very short-lived.

Wilson & Davison (379) cite psychophysiological research to argue that certain cues may be especially *appropriate* as conditioned stimuli for certain response systems. They conclude that chemical aversive procedures offer greater flexibility than do electrical procedures when you consider the *topographical* nature of a stimulus and a response.

The Lang & Melamed (205) case report has important theoretical and practical implications. They used aversive conditioning with a 9-month-old infant suffering persistent and ruminative vomiting for which no organic basis had been found and all therapies had failed to benefit. EMG recordings indicated that vomiting occurred within 10 minutes of intake and the onset of vomiting seemed to be signaled by distinctive throat movements. The treatment procedure involved 1-second electric shocks to the leg, sufficient to be painful, given at the start of vomiting and at 1-second intervals during vomiting. The vomiting stopped completely before the sixth session. Cessation of vomiting was accompanied by weight gains, increased activity level, and general responsiveness to people. A 6-month follow-up indicated no recurrence of vomiting and a healthy, normal child. These procedures can be fitted into either classical avoidance conditioning or operant conditioning paradigms.

Feingold (98) offers an automated technique for aversive conditioning in sexual deviations. Feldman et al (99) report on treating five patients with sexual deviations other than homosexuality by anticipatory avoidance learning. They are unusually candid in admitting that only two patients have responded successfully.

Carlin & Armstrong (59) present a study of smoking behavior based on the hypothesis that the favorable results of aversive conditioning therapies may be based on the *incongruence* between the patient's behavior and his

beliefs, once he is committed to a behavior change in contrast to a direct learning base. Finding that each of their (conditioning and pseudoconditioning) groups significantly decreased smoking, they concluded that factors of "belief, expectancy, and cognitive consistency" were important in behavior therapy.

The studies of "learned helplessness" (319) indicate that when dogs do not have an opportunity to make an instrumental response to an initial noncontingent intense shock and then are placed in an escape-avoidance training apparatus, they fail to escape or avoid shock although control animals can learn to do so. The authors argue that the animals have learned a *relationship* between their responses and the aversive shock. The dogs learned that they can't influence their environment. The helpless behavior does not occur if there is a 38-hour delay between initial shock exposure and escaping. These results may help to explain the clinical analog of passive behavior in the face of threat. The usefulness of a particular aversive stimuli for behavior change may depend on the individual's prior experiences with these stimuli and his prior ability to escape or avoid their impact.

Finally, in aversive procedures, consideration must be given to the relationship between the punisher and the punished. The variables of the social influence base are as basic to aversive conditioning as to other behavior therapy procedures. Studies reported by Patterson & Reid (267) with children in the family setting have emphasized the mutually coercive relationships which are maintained by the effects of relative noxiousness of control that family partners exert on each other.

Risely (293) noted that it is only after elaborate training that mothers shift from generalized aversive control to a specific positive control, demonstrating one of the paradoxes of our time. Society generally uses aversive procedures to control deviant behaviors, but at the same time deplors aversive control. The unfortunate fact which seems to emerge from much of the training work with parents is that most people are simply untrained in the use of positive reinforcement.

Cautela (61) presented an interesting aversive technique which involves the pairing of unwanted thoughts with imagined aversive stimuli (e.g. nausea). The technique has been successfully applied to, among others, problems of alcoholism (6) and sexual deviation (31).

Birnbrauer (45) discusses and evaluates the use of response contingent electric shock in the elimination of self-destructive behavior in a retarded adolescent. His study particularly demonstrated the limitations of shock punishment and is important in any future consideration of such procedures. Gardner's (115) review of "coercive" procedures with the severely retarded suggests that they may produce desirable behavior change without negative emotional states or disruptive social relationships. Lovaas & Simmons (228, 325) attempted to isolate some of the environmental conditions that control the self-destructive behavior of retarded and psychotic children.

Three other techniques derived mainly from the operant approach belong

within the aversive group: satiation, the excessive use of positive stimuli (290); time out, access to positive reinforcement being blocked contingent on occurrence of an undesirable behavior (52); and response cost, reinforcement penalties per response (373).

Response cost.—Weiner (372) introduced a response cost contingency in a bar press task. This procedure made the subject more proficient at performing a required task by quickly modifying a "maladaptive behavior pattern," defined as failing to produce as much reinforcement as another individual would receive under the same schedule of reinforcement. If punishment is defined as a consequence of behavior that reduces the future probability of that behavior, then response cost procedures are punishment (13).

There have been a number of ingenious uses of the notion of response cost (356). Harmatz & Lapuc (140) worked with obese male patients. They were put on an 1800-calorie diet and were weighed weekly. Those patients who failed to lose weight during the week forfeited a portion of their financial allotment. This response cost group did not differ initially in weight loss from other patients in group therapy or those in a diet-only control group. The important result was that those exposed to the response cost procedure continued to lose weight during a 1-month follow-up period, whereas the controls regained the weight they had lost.

Burchard (52) was the first to use response cost in a token economy as represented by the verbal phrases "time out" and "seclusion" (e.g., "time out" would cost four tokens). Phillips (277) used response cost procedures in setting up Achievement Place, a token economy for delinquents. The behaviors changed were aggressive statements, poor grammar, and lateness. The notion of response cost and its application in the classroom and in the mental hospital represents an important practical link between the laboratory and the clinic.

Experimental studies.—There have been a considerable number of studies and reviews of the experimental literature, particularly in the field of avoidance conditioning, which are relevant for behavior therapy.

Rescorla (289) offers a comprehensive review of experimental studies of Pavlovian conditioned inhibition defined in terms of the learned ability of a stimulus to control a response tendency *opposed* to excitation. He concludes that conditioned inhibitors are not generated either by simple extinction procedures or by pairing a stimulus with the termination of the unconditioned stimulus, rather conditioning is best described as arising from the contingencies arranged between CS and US; positive CS-US contingencies lead to the development of conditioned excitation and negative contingencies lead to the development of conditioned inhibition. Herrnstein (146) argues against a two-factor theory of avoidance conditioning.

Geer (117) presents a test of a classical conditioning model of emotion in

which nonpainful aversive stimuli (color photos of victims of violent death) were used as unconditioned stimuli and associated, either forward, backward, or randomly with conditioned stimuli (tones). Using GSR as the dependent variable, the evidence points to the usefulness of such a conditioning model.

Garcia & Erwin (113) review some of the traditional concepts of learning in light of recent evidence of their own studies and of other animal studies. Their conclusions are relevant for behavior therapy research and include the finding that "not all the stimuli which impinge upon the organism, and are followed by an effective reinforcer, acquire secondary reinforcing properties" and that "the neural mechanisms of learning and memory may differ depending upon specific sensory modalities used to signal the animal and to reinforce its responses" [p. 409].

Use of apparatus.—Azrin & Powell (14, 15) report on two pieces of apparatus using aversive principles. The first is a portable operant apparatus "based on response priming and escape reinforcement." The apparatus sounds a tone when a pill has to be taken. When the individual turns a knob to end the tone, the apparatus delivers a pill. The second is a cigarette case that automatically locks itself for a period of time after a cigarette has been taken from it.

MODELING PROCEDURES

Modeling and the use of vicarious processes in the acquisition and modification of patterns of behavior have become important techniques of behavior therapy. Bandura (25) concludes that:

research conducted within the framework of social-learning theory demonstrates that virtually all learning phenomena resulting from direct experiences can occur on a vicarious basis through observation of other persons' behavior and its consequences for them. Thus, for example, one can acquire intricate response patterns merely by observing the performances of appropriate models; emotional responses can be conditioned observationally by witnessing the affective reactions of others undergoing painful or pleasurable experiences; fearful and avoidant behavior can be extinguished vicariously through observation of modeled approach behavior toward feared objects without any adverse consequences accruing to the performer; inhibitions can be induced by witnessing the behavior of others punished; and, finally, the expression of well-learned responses can be enhanced and socially regulated through the actions of influential models. Modeling procedures are, therefore, ideally suited for effecting diverse outcomes including elimination of behavioral deficits, reduction of excessive fears and inhibitions, transmission of self-regulating systems, and social facilitation of behavioral patterns on a group-wide scale [25, p. 118].

The above summarizes a large range of research which only recently, through the efforts of investigators such as Bandura, Kanfer, Lovaas, Baer, and their collaborators, has become an integral part of behavior therapy. Research investigators in this area have used a variety of labels under which

to classify their work such as modeling, imitation, vicarious learning, vicarious reinforcement, identification, and social psychological terms including role playing, social facilitation and contagion (4, 6, 24, 26, 38, 105, 124, 143, 324).

The application of modeling procedures to the modification of specific kinds of behavior has occurred within two different but not mutually exclusive conceptual frameworks. Those working within the operant framework have tended to view the introduction of modeling procedures as an additional training technique to elicit behaviors. Baer (18) summarizes the approach to the use of imitation, particularly its use in eliciting verbal behavior in children. Baer describes the initiation of his research "not from a basis of clinical ambition to improve behavior, but rather out of a curiosity about the fundamental nature of imitation. Is imitation a type of learning qualitatively different from operant conditioning, or only a more complex organization of the results of operant conditioning than is the simple discriminated operant?" [18, p. 12].

On the other hand, a recent study of Bandura, Blanchard & Ritter (27) derives in part from the avoidance learning research, particularly of Solomon and his collaborators. The authors place their study within the "dual-process theory of avoidance behavior." According to this view, "threatening stimuli evoke emotional arousal which has both autonomic and central components. It is further assumed that these arousal processes, operating primarily at the central level, exercise some degree of control over instrumental avoidance responding" [p. 173]. Bandura, in a series of earlier studies (e.g. 24, 28), had demonstrated that avoidance responses can be extinguished if the individual is exposed to a graduated sequence of modeling activities beginning with those that have low arousal value (in effect vicariously eliminating fear arousal). Based on these earlier results, Bandura, Blanchard & Ritter administered either symbolic desensitization, symbolic modeling, live modeling combined with guided participation [a procedure called contact desensitization developed by Ritter (295, 296)], or no treatment control with 48 snake phobic individuals. The subjects in this study, in contrast to most "snake phobics," were individuals whose lives were restricted in some way because of their snake phobia. Although all three approaches produced reductions in fear arousal, the modeling with guided participation proved most powerful in achieving virtually complete extinction of phobic behavior in every subject. The favorable changes produced toward the phobic object were accompanied by fear reduction toward threatening situations beyond the specifically treated phobia. Bandura's research and general theories of identification (25, 26) are forging a major link between the experimental laboratory and real life modification of behavior.

The deliberate use of behavior of the therapist (or an individual trained by him) as an example, illustration, or model to influence the behavior of another person has occurred in the one-to-one interview (358), has been combined with reinforcement in training a retarded child (276), has been used as an "observational learning opportunity" with juvenile delinquents

(309), has been used via a film depicting social interaction between children to enhance social behavior in preschool isolates (257), has helped decrease fear of dogs in preschool children by filmed models (149), and has been used in treating snake phobias (292).

OTHER PROCEDURES

Control of autonomic functions.—One of the most important recent developments both theoretically and practically has been the demonstration that unelicited autonomic responses can be strengthened by reinforcement after their emission. Techniques have been developed for the direct training of autonomic systems through exteroceptive feedback and operant shaping techniques. Lang (204), in the most comprehensive review of the research of autonomic control, credits Shearn (321) with first demonstrating the operant conditioning of heart rate acceleration. Shapiro et al (320) suggest that blood pressure may be brought under control via feedback and reinforcement: Lang and his associates (207) have also reported that heart rate may be stabilized within narrow limits when feedback is provided. Sroufe (338) obtained similar results with respiration experimentally controlled. Ascough & Sipprelle (5) demonstrated that spontaneous increase in heart rate can be brought under control of operant verbal conditioning.

States of psychophysiological relaxation can be produced by the use of electromyograph feedback from striate muscles (346). Kamiya (166) demonstrated that people can be taught to control some of their own brain wave patterns by hearing a feedback buzzer whenever the desired pattern is occurring. They learn to associate their subjective mental state with the buzzing so that by reproducing that mental state they can reproduce the brain wave patterns whether or not the buzzer is on.

Paul (271) demonstrated that with undergraduate females, relaxation training (and hypnotic suggestion) produced significant *arousal* effects in physiological measures (heart rate, respiratory rate, tonic muscle tension, and skin conductance). Craig & Lowery (74) report the vicarious classical conditioning of both GSR and heart rate by the use of electric shock to a model as the UCS.

The animal experiments of Miller and his associates (249, 250) on curarized rats have provided the most sophisticated convincing demonstrations of operant control of autonomic functions. Miller argues that psychosomatic conditions may develop through contingent attention and other reinforcing consequences. It should then be possible to modify the visceral responses that occur in psychosomatic disorders by extinction and differential reinforcement.

In an illuminating series of papers, Katkin & Murray (178) offer a critical and skeptical review of the methodology of the instrumental conditioning of autonomically mediated behavior. Crider, Schwartz & Shnidman (76) refute the earlier critique, and Katkin, Murray & Lachman (179) reiterate their criticisms. The initial review stressed "such methodological

shortcomings as inappropriate controls and systematic biasing effects, and focusing on alternative explanations of positive findings." Crider et al argue for *direct* instrumented strengthening of autonomic activity and against cognitive explanatory concepts. They defend the frequently used yoked-control designs in such studies. Katkin et al concede many of the points made by Crider and associates but summarize their arguments that instrumental autonomic conditioning in humans is "far less convincing" than evidence from animal studies.

Self-control.—The term self-control has found increasing use among behavior therapists (127). One way of utilizing it has been to represent internal mediating events. However, in a broad sense all of the behavior therapy procedures may be viewed as training in self-control conceived of as the individual regulating his own behaviors by arranging appropriate contingencies (63). Kanfer & Duerfeldt (171) compared self-reward and self-criticism as a function of external reinforcement. Kanfer (170) reviews his considerable contribution to research in self-regulation.

Behavioral counseling.—A new approach to counseling developed in the late 1960s to which the term "behavioral counseling" has been applied (195, 196, 354). The basic principles and techniques are identical with those of behavior therapy, the only difference being that the targets of the behavior counselors usually are children, adolescents, and college students who are relatively intact. Most of the studies avowedly in this area are case reports involving unusual techniques.

Krumboltz & Thoresen (196) describe techniques which involve the use of teachers and friends. Hosford (158) taught teachers to reinforce shy students for participating in class discussions. As the students changed their class behavior, the teachers changed their teaching behavior. Daley (78) describes a reinforcement menu (a picture book of 22 activities from which a child can select a reinforcer) to determine appropriate reinforcers for 8 to 11-year-old retardates. Keirse (181) used behavior "contracts" with school children, with the reinforcer being permitted to remain in school. Social modeling is a frequent technique in behavior counseling. This includes the use of symbolic models in the form of recordings, films, videotapes, and physically present models (196). In these modeling studies difference in sex between model and subject may affect degree of imitation. Adolescent models have strong social power or prestige as imitation models for their peers (29). Subjects are more prone to imitate the behavior of high status models than models similar to themselves (355).

Miscellaneous.—The one group of procedures not previously mentioned is the combination of any of the above techniques with drugs of various sorts. Brady (47) has reviewed the growing literature of research in combining drugs with relaxation and desensitization procedures. Davison & Valins (81) report an experimental analog of drug therapy in which subjects

who attributed behavior change to themselves (they believed they had been given a placebo) were better able to tolerate shock than subjects who attributed behavior change to a drug (183).

ASSESSMENT

The process of assessment is an integral part of behavior therapy. However, the goals and purposes of assessment are quite different from that of traditional psychotherapy consistent with the differences in theoretical models. The implications of these differences are summarized by Mischel (251) as follows:

A focus on behavior change, rather than on stable dispositions . . . leads to quite different emphasis in assessment and in research. In behavioral analysis the emphasis is on what a person *does* in situations rather than on inferences about what attributes he *has* more globally . . . social behavior assessments seek behavioral referents for the client's complaints; thereafter they identify the precise conditions that seem to be maintaining and influencing these problems so that appropriate rearrangements can be designed to achieve more advantageous outcomes. . . . Behavior assessments therefore often have to begin by establishing clear referents for highly subjective problems and goals [p. 280].

The behavior therapist is a gatherer of data which he and his client can use as the basis of behavior decisions. Instead of asking the usual "why" or "how" questions, the focus is on the "what" questions, e.g.: *What* are the conditions under which a particular behavior is emitted? *What* alternative behaviors should be taught? In addition to Mischel's book there have been several comprehensive reviews of assessment (62, 126, 374) or "behavioral diagnosis" (174) which have focused on this vital aspect of behavior therapy.

In addition to the contribution of a new way of approaching the assessment process, there has been a growing variety of specific assessment techniques related to specific modification procedures (131). Bentler (36) describes a sexual behavior scale for males consisting of 21 classes of heterosexual behavior forming a cumulative ordinal scale which can be used either as a hierarchy in desensitization or can be used to assess the change itself. Cautela & Kastenbaum (64) introduce a Reinforcement Survey Schedule (RSS) which could be useful in determining what environmental events an individual considers to be rewarding to him. Suinn (350) presents a scale for measuring test-taking anxiety. Lader, Gelder & Marks (199) suggest the use of autonomic measures such as palm or skin conductance as predictors of response to desensitization.

MODELS OF DEVIANT BEHAVIOR

The paradigm conflict between behavior therapy and traditional therapeutic procedures is most clearly seen in the contrast between the models of behavior deviancy which each espouses. Basic to the traditional model is the notion that *underlying* causes of pathology must be extirpated to achieve permanent behavior change. Unless these basic "causes" of pathology are

removed, a change in overt behavior would only result in symptom substitution. From the earliest days of behavior therapy the theoretical implications of the notion of symptom substitution have been a controversial issue (96). Cahoon (57) offers a reappraisal of the concept in an attempt to state it in such a way that it may be empirically investigated. Baker (20), in one of the few studies specifically investigating this area, reports that he did not find evidence for symptom substitution after treating 30 enuretics by conditioning procedures.

Recent formulations alternative to the disease model are worth noting. Albee (3) presents a social-developmental model which emphasizes the strength of an individual. While not specifically identified with behavior therapy, Albee's formulations have had considerable impact on the broader aspects of training of the clinical psychologist. Scheff (313) offers a model of mental illness in terms of norms, rule breaking, deviance, and social role taking, emphasizing the social consequences of the labeling process. Ullmann & Krasner (361) offer a similar social psychological formulation.

ON SPECIFIC PROBLEM BEHAVIORS

Almost every conceivable human problem behavior has been approached by the behavior therapist. This section will focus on several specific problems investigated by recent research. A group of investigators reported success with modifying depressive behavior which until recently has been infrequently handled within the framework of behavior therapy (53, 219). It was Ferster (100) who first noted that the loss of a close friend or relative constitutes a sudden shift or reduction in the *schedule of reinforcements* maintaining many of an individual's behaviors, and that a reduction in the rate of the person's behavior would be a logical consequence following the death of a close relative or loss of his source of social reinforcers. More recently, Lazarus (210) conceptualized depression as a response to "inadequate or insufficient reinforcers." The approach of these investigators to depression seems promising. They use home observation, focusing on behavioral and interpersonal problems, and involving the client's environment in the treatment.

Recent investigations view overeating as a *learned behavioral excess* and in that respect similar to alcoholism, smoking (42, 184, 221), drug addiction, or fetishism (140). An earlier and predominant behavior therapy approach to such behavior has been aversive conditioning in which the stimuli for the undesirable behavior was repeatedly paired with noxious stimuli until they elicited avoidance behavior by themselves. In contrast, the more operant influenced investigators have emphasized the training of subjects to monitor and control their own behavior by making reinforcement contingent on such control. Even auditory hallucinations may eventually come under such control (241).

Implicit in the behavior therapy approach has been a reconceptualization of the group of behaviors generally labeled as schizophrenia from that

of being a product of disease to that of a product of social learning. Three facets of the research in this area have been (a) to demonstrate the modifiability of specific schizophrenic behaviors such as abstract thinking (77, 246, 368); (b) to analyze the behavior of schizophrenic patients in terms of their success in "impression management" and manipulative tactics (48); and (c) to demonstrate the reproducibility of schizophrenic behavior in normals (218). Rosenbaum (297) argues that this approach implies that schizophrenia is a "put on," but the behavior therapist would contend that the schizophrenic role is no more of a "put on" than any other *learned* social role.

Leff (214) reviews behavior therapy as applied to the "psychoses" of childhood, particularly autism. He uses Ferster's concept of behavioral deficit as the theoretical framework upon which much of the later applications are developed. He concludes that "despite some lack of experimental rigor the evidence is interpreted as strongly supportive of a social-learning model of severely pathological behavior and as an operant-training model of therapy." Wolf et al (385) illustrate the operant approach to autistic children.

Populations.—An examination of the literature reveals a trend on the application of behavior therapy, insofar as populations are concerned, in the opposite direction from traditional psychotherapy. In contrast with the initial focus of evocative therapy on the YAVIS (young, attractive, verbal, intelligent, successful) (314) the behavior therapist started with the unattractive, the mute, the retardate, the one on whom everyone else had given up. For example, it is in the field of working with mental retardates that behavior therapy first had a major impact and has advanced to the point where it is "the treatment of choice" (88). In contrast, it has only been relatively late in the history of behavior therapy that the YAVIS are getting their just due as behavior therapy moves into the counseling area (196), as desensitization becomes appropriate to the full range of "neurotic" disorders (387), as token economies move into the home (349), as the social psychological principles predominate in the approaches to the problem behaviors of everyday life (129, 361). The field of physical rehabilitation has also been strongly influenced by behavioral techniques (100).

ETHICAL ISSUES

Concern for the ethical implications of a scientific endeavor is, of course, not unique to behavior therapy, but is one of the outgrowths of the role of science in the atomic age. But behavior therapy, because of its promise and the possible social threat implicit in its effectiveness, has forced both society and the helping professions to directly face the issue of behavior control and ethics. There have been numerous papers and books exploring the implications of behavior control (168, 190, 193, 362). London's (226) review of behavior control is an updating and extension of his earlier ethical analysis of all forms of psychotherapy. Schwitzgebel (315) issues a call for responsible

use of "behavioral electronics." Lucero, Vail & Scherber (230) report the reactions in Minnesota to the use of a deprivation procedure in a state hospital token economy program. All of the recent books (described in a previous section) which offer an overview of behavior therapy devote at least a chapter to the discussion of ethics and values.

Within the ethical overview must also be included the interest in and concern with the design of social environments to influence individual behavior. In his presentation of a utopia (327), Skinner foresaw at a very early stage where the behavior therapy formulations inevitably would lead. The implications of these viewpoints have been extended to the general problems of the design of cultures and "freedom" (331, 332).

RESEARCH DESIGN

The behavior therapy researcher uses a number of procedures and designs to go about acquiring information. These are: extrapolation from the experimental lab to clinical application; the use of the clinic as a source of hypotheses for laboratory research; the use of research as treatment; the single case as an experimental model (the subject becomes his own laboratory and hypotheses are tested solely with reference to that individual); and the individual as his own control.

The research of behavior therapists includes the usual traditional design involving control groups as well as the use of *own-control* groups particularly by those investigators utilizing operant procedures. There have been several excellent critiques of experimental procedures of behavior therapists. In his comprehensive review of the design and tactics of behavior modification research, Paul (272) concludes that:

the principles and methods of behavior modification research appear to be no different than any other experimental design problem, except for the greater number and complexity of variables . . . the research designs considered . . . included: individual case studies and nonfactorial single-group designs, ranging from simple case reports through more sophisticated time-series and time-samples designs; nonfactorial designs with untreated controls; multiple-treatment designs; factorial group designs with untreated and nonspecific-treatment controls; and laboratory-based designs. Although factorial designs were shown to be capable by themselves of establishing cause-effect relationships between specific variables of importance in behavior modification research, an inverse relationship exists between the level of product and the necessary investment of time, money, and personnel [pp. 61-62].

The Gelfand & Hartmann (121) critique of research methodology of behavior therapy with children suggests that in therapy evaluation studies the experimenter should provide: adequate baseline measures of the target behavior, systematic variation of reinforcement contingencies or other procedures demonstrating control of the subject's behavior; evidence that behavior observations are unbiased; and rigorous follow-up evaluations. Gelfand & Hartman then conclude:

Unfortunately, many of the behavior-therapy studies reviewed in this paper fail to meet most of the assessment standards suggested in the evaluation paradigm and thus represent no improvement over the traditional clinical case study in terms of experimental rigor. Nevertheless, in contrast to the play-therapy case-study literature, there are a small but growing number of carefully designed behavior-therapy case studies which meet most, if not all, of the suggested evaluation criteria and which convincingly demonstrate the power and efficiency of behavioristic treatment approaches [121, p. 212].

A critique and evaluation of behavior therapy with mental retardation with similar conclusions is offered by Gardner (114).

CRITIQUES OF BEHAVIOR THERAPY

Behavior therapy has generated a full range of criticism, logical and illogical, dispassionate and passionate, with and without experimental evidence, from within and from without. The most frequently cited critique, that of Breger & McGaugh (49), has been repeatedly responded to in detail by, among others, Rachman & Eysenck (285), Wiest (376), and the authors of the recent books cited in the earlier section. Many of the criticisms implicitly offer a theoretical model of behavior therapy which is unrelated to reality. Some of the criticisms may be briefly summarized as follows: Behavior therapy is only concerned with symptom removal; it only deals with simple behaviors; it is too mechanical; in being ahistorical it misses important data about individuals; it ignores biogenetic variables; there is a spurious relationship between basic laboratory findings and clinical applications based on analogy rather than on fact; there is no "modern learning theory" upon which behavior therapy is supposed to be based; social learning models of behavior are inadequate representations of behavior pathology; behavior therapy has not demonstrated its effectiveness; behavior therapy is too effective, hence dangerous; it is a useful technique only for phobias; it is merely a collection of case studies; when behavior therapy is effective it is due to interpersonal relationships, placebo, or suggestion. These represent only a partial list of the kinds of criticism leveled against behavior therapy. Many of these are based on misconceptions about, and a very narrow view of, behavior therapy which is frequently reinforced by behavior therapists themselves.

Yates (395) argues that the misconceptions about behavior therapy derive in large part from statements of its *proponents*. He particularly emphasizes the aversive consequences of Eysenck's (92) definition of behavior therapy as "the attempt to alter human behavior in a beneficial manner according to the laws of modern learning theory." Yates views behavior therapy as a "*methodological approach*" (the use of experimental method in the study of the individual case) (p. 402). Yates attributes further misconceptions to the Eysenck & Rachman (97) position that identifies abnormality of behavior with neurosis and psychosis. Yates would extend behavior therapy to *abnormalities of behavior* even in "essentially normal persons."

Unfortunately, this position itself perpetuates the disease model and only an alternative social labeling model of deviant behavior would permit behavior therapy to be completely consistent. This critique of a critique of a critique within behavior therapy may help explain why many criticisms are justified yet based on a myth which offers only a part of the elephant. Many of the criticisms based on a limited perspective of behavior therapy can be clarified by presenting the overall picture of behavior therapy; others, however, point up the still tenuous nature of some aspects of the field.

To give a flavor of recent comments which cover much of the same ground as above, the following may be cited. Freeman (111) criticizes the theoretical base of behavior therapy from a psychoanalytic viewpoint on the grounds that it is "over-simplified." Klein et al (185) offer a critique of behavior therapy as observed by a distinguished group of evocative therapists. One of their concerns is that behavior therapy does not take sufficient cognizance of the role of "suggestion" in the patient-therapist interaction. In effect their reaction was "So what's really different?" Strupp & Bergin (348) argue that *all* major approaches to therapy use a small number of specific mechanisms of psychological influence-imitation, identification, persuasion, empathy, warmth, interpretation, counterconditioning, extinction, discrimination learning, reward and punishment. The aim is to determine the relative contribution of each process and the extent to which it can be employed. Hence, the claim of behavior therapy as unique cannot be upheld. This is a sophisticated version of the "bridging," or "there are really no differences," viewpoint.

From within the behavior therapy framework, Evans & Wilson (91) offer an important note on the confusion between behavioral *events*, the *conditions* producing these events, and the *explanation* of these events. This kind of confusion is especially notable about the terms reciprocal inhibition, counter-conditioning, and systematic desensitization. The authors present a good historical and logical argument that counter-conditioning as a term should stand for extinction or response decrement; systematic desensitization should refer only to the treatment technique evolved by Wolpe; and reciprocal inhibition should be used only as a molecular explanatory concept.

There are also the kinds of critiques which offer theoretical models that are alternatives to previous ones. For almost every procedure there are at least several alternative explanatory concepts (e.g. see section on desensitization). There are critiques of specific aspects of research designs. For example, Cooper et al (71) question the use of snake phobias as an *analog* of clinical neuroses. In their study they demonstrated that subjects would hold snakes after merely being *told* to hold them and to "act unafraid."

THE FUTURE OF BEHAVIOR THERAPY

It is likely that most of the trends of research and application now discernible in behavior therapy will continue at least through the near future. The research base of behavior therapy should become even more solid

as sophistication increases in design and equipment. The linkage between "basic" research and "real life" should become firmer but will probably remain as controversial as it is now.

Behavior therapy research, and the ability to measure behavior, will go further *inside* the organism by relating autonomic and neurological functioning with the behavior change process, and further *outward* to the multivariables of the social environment. This latter direction could be labeled as a "social movement" (359), thus changing the role of the psychologist from that of a passive reactor to social pressures to that of active agent of social change. This will be partly mediated by his training a greater variety and number of people in behavioral principles and in the implicit value system that accompanies them. There will be an increase in research involving a functional analysis of social systems analogous to that now done in the schoolroom, the hospital ward, and the home in setting up token programs. This approach will link with the content areas of fields such as economics, sociology, and political science which involve social and economic observations and planning. There will be a further extension of the principles and procedures of behavior therapy into the social systems of the community—e.g. in schools, hospitals, welfare agencies, courts—both to *observe* and to *change* the systems. It is obvious that the behavior therapy model will have increasingly important implications for the training of clinical psychologists as well as all other professionals in the "mental health" field (193).

Eventually there will be a unified theory which will link man's social, economic, political, and ethical behavior within a social learning framework. However, such a theory is still in the very early stage of *identifying* the variables of social influence. Its development will not be the panacea for all men's ills, but it will open the way for broad social intervention and planning which is *preventative* in nature. Homans (154) argues that "the propositions of behavioral psychology are the general explanatory propositions not only of sociology but also of all the social sciences."

Mowrer (253) includes among his approaches to modifying behavior "ecology," or social planning for populations, as well as re-education for "bad choices and habits for individuals." Mowrer's emphasis on the development of "self-help" groups foreshadows the extension of behavior therapy into this kind of community activity (e.g. 248) with the focus on developing individual responsibility in group settings.

An important facet of the development of behavior therapy is its movement into other fields and disciplines such as social work, sociology (153, 354), economics and exchange theory (89, 123, 256), and even college teaching (182). Craik (75) opens an exciting vista into the contribution an "environmental psychology" can make to the interaction between transportation systems and behavioral systems. Karen & Bower (176) offer a behavioral analysis of Synanon as a social control agency, pointing up the similarity of the procedures of this agency to those of large scale behavioral programs such as token economy.

One is impressed with the relevancy, in terms of social living, of a considerable amount of research in behavior therapy. The principles of scientific rigor have, more or less, been maintained in most of the studies in this field, but as the complexities of the problems investigated increase, particularly as they encompass more variables of the social environment, it becomes increasingly difficult to demonstrate control of all but the one relevant variable being investigated. Moreover, unless the behavior therapist is able to maintain his scientific cool with its strong ethical base, he will become a cultist and a dangerous one at that since his procedures can also be utilized for coercive social control. The potential for productive social influence is there and perhaps will win out, but not without a concerted effort by the behavior therapists to maintain their current momentum.

The decade of the 1960s covered the childhood and adolescence of behavior therapy (its birth was in the 1950s). The 1970s should see its development into adulthood and perhaps even maturity. Ahead will almost certainly lie old age and senility in the 1980s, but by that time it will have given birth to a newer and at this point (at least by this observer) unpredictable paradigm.

As for the future relationship between behavior therapy and the *Annual Review of Psychology*, the plethora of verbiage now extant within behavior therapy could call for a continual stream of chapters on *specific* research procedures such as desensitization, positive reinforcement, aversion, or modeling.

SUMMARY

This chapter has presented an overview of the field of behavior therapy first by putting it in its historic context and second, by presenting the diverse elements of research and clinical practice which are necessary to label and differentiate this field from other approaches to changing human behavior. Behavior therapy is conceptualized as the application of behavioral principles derived from the experimental laboratory to modifying human behavior labeled as deviant. Behavior therapy involves the usage of specific techniques, usually based on learning principles, within the context of a social influence situation. A review of the literature of behavior therapy indicates that the present status of the field is comparable to that of most other fields of psychology: a plethora of publications, many inadequate research designs, some well-controlled sophisticated studies, an urgency for application to relevant human problems, enormous promise as to its efficacy in changing behavior, great need for caution, and, perhaps above all, the need for ethical concern as to its social and value implications.

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STUDENT DEVELOPMENT AND COUNSELING 173

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The collegiate experience provides for a student the opportunity to discover and try out new ideas; to question his and others' attitudes and values; to experiment with new personal, social, educational, vocational, and sexual roles; to decrease dependence on home and family; to enjoy increased competition in the academic arena; and to associate with a variety of new and different people. Higher education can and should be stimulating, stressful, and growth producing for students who experience it. The counseling psychologist, as educator on the one hand, should help the educational institution to provide those experiences which will be developmental to students and, as a counselor on the other hand, should help the student to cope with the stress of being a student.

Most students handle the stimulation of college reasonably well without professional help and thrive in the university environment. However, the stress of the college experience causes students to reassess themselves, their abilities, identity, values, and feelings, so all students may benefit from counseling. Especially, counseling can be beneficial to those persons whose habitual modes of coping with stress are defense mechanisms which interfere with growth processes. For example, students who cope by withdrawing, by becoming passively dependent, by acting out, or by being overly aggressive, will not derive maximum benefit from college.

The literature reviewed in this chapter was selected for its relevance to the counseling psychologist doing research on students and providing services to them. The topics deemed relevant are: student development including vocational behavior and development, counselor training, counseling research methods, counseling process research, and challenges to counseling.

STUDENT DEVELOPMENT

Counseling psychologists, by understanding students and how they develop, can facilitate the growth of students by providing individual and group counseling and other developmental programs. Student development is viewed by the reviewers as the product of person-environmental interaction. It is the result of the interaction of student characteristics, including

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expectations of college, and the opportunities, demands, restrictions and sanctions, concerns and indifferences of the college environment and its subcultures. The processes used by the student to deal with the environment and stresses it produces can reveal much about his attitudes, values, and habitual modes of coping. Maturation of the student depends on his using environmental interactions as personal growth experiences. If we accept the view that student development results from person-environmental interaction, then we should accept the need to define the interaction in multivariate terms with multicharacteristic persons reacting to multicharacteristic environments with personal development resulting. In order to develop programs and methods, as well as theory, counselors and student personnel workers in the college setting will need to adopt models of human behavior which take into account not only qualities of the individual student, but of the setting in which he operates and the behavior change program in which he is engaged.

Mitchell (68) lists five criteria for a research methodology for educational psychology that has potential for contributing to our understanding of student development. These five are: 1. conceptualization of research problems whenever possible within a framework of dynamic person-environment-interactional systems; 2. providing methods for defining and assessing person-environment relationships that are theoretically and operationally sound; 3. providing techniques for measuring critical environmental and situational variables with reliability, validity, and precision at least as good as those techniques now employed for measuring personal variables; 4. casting of research designs in multivariate terms that reflect the complexity of both personal and environmental domains; 5. application of appropriate multivariate statistical methods for analyzing the data derived from the methods.

Research methodology must also be accommodated to the characteristics of the developmental process: development is continuous, irreversible, differentiable into patterns, and in any given time segment some aspect of the process is pre-eminent.

The time span to be included in the study of student development is perhaps of crucial importance, for the environmental influences must have time to produce relatively stable (reliable) effects. As Katz (53) indicates, some factors dating back to early childhood may influence the college student's coping behavior. Longitudinal research which covers the developmental stages of adolescence and youth may be most efficacious in conceptualizing development of student characteristics such as social, sexual, and vocational maturity, but there should be awareness that development occurs over a longer time span.

Youth has been defined by Keniston (55) as a postadolescent stage whose occupants have completed the tasks of adolescence in Erikson's terms (emancipation from the family, comfort with sexuality, attainment of a sense of identity and of a capacity for intimacy), and who postpone accep-

tance of adult responsibilities during a period of experimentation with adult roles, and who continuously assess and reassess their personal relationship to society.

It is clear that *student* development occurs and occurs to a greater extent in students than it does in nonstudents. Trent & Medsker (84) drew the following conclusions from a longitudinal study of 10,000 post-high school persons throughout the United States during the period 1959-63: personality development, whether in terms of reported change in values or in measured attitudes, took place most among college persisters, next most in withdrawing students, then in employed young people, and least of all among the homemakers who experienced neither college nor employment during the first 4 years after high school. Obviously, the objectives of higher education are based on the assumption that personal development can be influenced.

But what are the aspects of student development and of student environment that are subject to discernment, abstraction, and naming which should be defined to meet the purposes of the researcher, the teacher, the educational administrator, and the counselor? Given the answers or partial answers to these questions, it is appropriate to ask the significant question posed by Newcomb & Feldman (69), "Under what conditions have what kinds of students changed in what specific ways?"

Students can be asked to respond to statements about themselves, their environment, and its impact on them. The Stern Activities Index and College Characteristics Index (Stern 79) exemplify the approach of garnering students' responses to items reflecting possible perceptions of themselves and their environments. Stern's approach is based on a conception of personal needs and environmental press, as we shall see below. An important question is, "What are the concepts the students themselves use in perceiving and describing themselves, their environment and its impact?" In this vein Layton, Wolins & Berdie⁴ asked, "What are the concepts used by adolescent boys in describing themselves?" and answered the question (defined the concepts) by factor analyzing the boys' responses to the Minnesota Counseling Inventory. Some of the 26 concepts defined were: Willingness to Admit Maladjustment, Social Introversion-Extraversion, and Intrapunitive Withdrawal. Faculty and administrators can be asked their perceptions of students, environment, and environmental impact. Other more objectively determined indices may be obtained by administration of tests to students and collecting demographic data on students and their environments (e.g. parents' education and size of institution).

The research of Elton & Rose (34) assessed characteristics of freshman students in four different institutions with the Omnibus Personality Inven-

⁴Layton, W. L., Wolins, L., Berdie, R. F. 1969. *Factor Analysis of the Items of the Minnesota Counseling Inventory*. Unpubl. manuscript. Ames, Iowa: Office of Student Affairs, Iowa State Univ.

tory, and compared the results with those of Astin, who inferred the personality of the freshman classes at these institutions in *Who Goes Where to College* (7). They demonstrated the contradiction that can result from using two different methods of inferring characteristics of students at institutions. The Astin approach used vocational choices of entering freshman and self-reports of high school achievement in art, literature, and music to determine input factor scores which had sizable multiple correlations with 24 items of institutional information. This information served as a basis for developing estimated factor scores from which the dominant personality characteristics of student bodies, including the institutions studied by Elton & Rose, were inferred.

Panos (71) has suggested some criteria for student development. He stated that research problems involving the outcomes of higher education should include student behaviors relevant to educational goals and relate those behaviors to the dynamic influences in the college environment. Panos believes that the statements of educational objectives articulated at the level of higher education are too global and too abstract to function as a source of viable conceptual criteria, so he has developed a scheme for classifying types of criteria of student development. In his framework, cognitive observable behaviors are educational attainment, vocational choice, skills, and achievements; and effective observable behaviors are avocations, personal relationships, mental health, and citizenship. Examples of effective hypothetical constructs are self-concept, need achievement, values, and attitudes; and examples of cognitive hypothetical constructs are abilities, intellectual aptitude, and knowledge.

Two recent books summarizing a large amount of research on students and college environments are those of Stern (79) and Newcomb & Feldman (69). They present the variables preferred by these investigators. The variables defined by Stern to conceptualize persons and environments are derived from Henry Murray's approach to personality.

Stern's (79) book reports a very large amount of research by himself and his colleagues on the tools developed for describing the characteristics of students and college environments in terms of comparable psychological dimensions. The two instruments based on Henry Murray's theory of personal needs and environmental press are the Activities Index (AI) and the College Characteristics Index (CCI). Each Index consists of 300 items distributed among 30 scales of 10 items each. The AI scales parallel those of the CCI, one corresponding to behavioral manifestations of the various needs variables, the other to environmental press conditions likely to facilitate or inhibit their expression. Twelve personality and 11 environmental factors have been defined through factor analyses of the 30 scales of each Index. A refactorization defined four second order personality factors and three environmental dimensions. The four personality factors are: Achievement Orientation, Dependency Needs, Emotional Expression, and Educabil-

ity. Educability is of lesser magnitude than the first three and is made up of an overlapping of Achievement Orientation and Dependency Needs. The three environmental dimensions defined are Intellectual Climate (the counterpart of the Achievement Orientation dimension of the AI), Non-intellectual Climate (combines the Dependency Needs and Emotional Expression dimensions of AI as a counterpart), and Impulse Control.

Stern (79) presented the doctoral dissertation of Cohen (29), who studied college and university cultures through a factor analysis of both Activities Index (AI) and College Characteristics Index (CCI) scores of students at 55 colleges. The resulting five factors define dimensions of college and university cultures. The five dimensions are Expressive, Intellectual, Protective, Vocational, and Collegiate. Stern, summarizing the research reported and discussed in his book, presents common environment parameters and infers six basic environmental factors largely invariant from situation to situation and instrument to instrument. A seventh factor is also inferred but it has some association with situation- or instrument-specific scales. The seven factors are divided between Development Press Factors (Intellectual Climate, Achievement Standards, Group Life, and Personal Dignity) and Control Press Factors (Orderliness, Constraint, and Practicalness).

Thus Stern and his followers have established an interesting frame of reference of person-environment interaction based on students' perceptions of themselves and their environment. The stage is set for research on student development based on this frame of reference.

Newcomb & Feldman (69) consider five kinds of variables as clearly associated with changes in students: 1. colleges' public image, 2. student characteristics at entrance, 3. the nature of the college environment, 4. institutional size, and 5. homogeneity of the student body.

In their two-volume work, Newcomb & Feldman review and analyze 40 years of research on college students, with a particular emphasis upon the impact of the college experience on students. The 11 chapters in the book cover some of the following topics: change and stability during college, the diversity of major fields, impact of residence groupings, student culture and faculty, and persistence and change after college. There is a very worthwhile chapter on problems of interpretation in studies of this nature.

They review the change that has taken place among college students on many campuses around the country on the Allport-Vernon-Lindzey *A Study of Values*. They conclude that the strongest and most consistent changes have occurred on the religious and aesthetic scales. Scores on the aesthetic scale are higher for seniors than for freshmen. Religious values are of less importance to seniors than to freshmen. For the other four values—theoretical, economic, social, and political—there are clear inconsistencies in the direction and significance of change in the students.

They emphasize the importance of knowing the public image of colleges for a given college may have a public image such that it attracts students

who already fit the image. Consequently, there is little room for students to change.

According to Newcomb & Feldman, the most salient changes in students are an increasing open-mindedness and a decreasing conservatism in regard to public issues. There is a growing sensitivity to aesthetic and inner experiences. Students exhibit a declining commitment to religious values and increases in intellectual interests and capacities, with increases also in independence, dominance, confidence, and in readiness to express impulses.

They clearly express their preference for longitudinal versus cross-sectional studies and view the Trent & Medsker (84) study of 32 high schools in 16 communities, focusing on noncollege groups as well as college students, as an effective research method.

In their chapter on Sequence of Experiences, Newcomb & Feldman emphasize differences among freshmen, sophomores, and seniors. This conclusion seems to fit their findings. Freshmen have unrealistic and exaggerated notions about college, while sophomores tend to be satisfied with the college, and seniors tend to be less involved in and identified with their institution.

In the chapter on the Diverse American College, Newcomb & Feldman state that certain types of colleges are predominantly peopled by certain kinds of students. Non-authoritarianism, various intellectual dispositions, and political liberalism, as well as high status family background and high academic capacity, tend to characterize, in decreasing order, students entering private universities, public universities, private colleges, public colleges, and junior colleges. The more prestigious the institution, the more likely it is to attract and admit those students who have already most nearly attained the characteristics of "an educated man."

Some of the other major findings of Newcomb & Feldman are the following: 1. Freshman to senior changes in several characteristics have been occurring with considerable uniformity in most colleges in recent decades. These include declining authoritarianism, dogmatism, and prejudice; decreasing conservative attitudes; and growing sensitivity to aesthetic experiences. 2. The degree and nature of different colleges' impacts vary with their student inputs—that is, entering student characteristics which differ among colleges in patterned ways. 3. Within the same college, experiences associated with the pursuit of different academic majors typically have effects over and beyond those that can be accounted for by initial selection into those major fields. 4. Though faculty are often individually influential, for instance in career decisions, college faculty do not appear to be responsible for campus-wide impact except in settings where the influence of student peers and faculty complement and reinforce one another. 5. Conditions for campus-wide impacts appear to have been most frequently provided in small residential 4-year colleges, but perhaps this is an artifact because changes are easier to detect due to group homogeneity and obviousness of identification. These conditions include relative homogeneity of faculty and students together with the opportunity for continuing interaction, *not*

exclusively formal, among students and students and faculty. 6. Attitudes held by students on leaving college tend to persist, particularly as a consequence of living in a postcollege environment which supports those attitudes.

Katz (53) and his colleagues studied the freshman classes entering Stanford University and the University of California, Berkeley, in the fall of 1961. The variables chosen for study were six scales from the Omnibus Personality Inventory, the Authoritarian and Ethnocentrism Scales, and an experimental Attitudes to Drinking Scale. In addition, a questionnaire administered to and longitudinal interviews conducted with selected student samples throughout their 4 college years investigated moral, religious, and political values, peer relationships, and sexual development. Katz, in reviewing the results of the research, states:

The picture that emerges is that of a wide variety of patterns in which individuals react and develop during the college years. The college environment is a highly controlling one and it is so to the point of stress for many students, but some individuals have the psychological equipment not to let the formal and informal requirements of the structure interfere with their own individual development and they can utilize both the opportunities and even the obstacles of their environment for the purpose of their own growth. At the other extremes are those whose needs for passivity and for being told what to do have become so ego-syntonic that the comfort of orderly security does not make them experience requirements as either stressful or inhibiting. In between are the bulk of people whose lives never reach an adequate expression of their potential because they are handicapped by (1) inadequate self-awareness and inadequate self-assertion and (2) an environment whose demands and constraints are both not sufficiently elicitive of their potential and too thwarting to their spontaneity.

Katz' results in summary are: 1. The academic and intellectual offerings of the college do not mesh with the motivations of many students. (Both adequate learning and personal involvement in the process of intellectual inquiry suffer as a result.) 2. Many students do not learn to engage their reasoning capacities in solving problems they face in their own development. 3. The college years do not bring sufficient opportunity to develop the nonintellective parts of their character for many students.

Astin (5) described the kinds of data the American Council on Education will be accumulating over a longitudinal period. They are (a) Administrative characteristics: sex, type of control, geographical region, size, wealth, and selectivity. (b) Environmental stimulus factors: peer environment, classroom environment, geographical environment and living quarters, and administrative environment. (c) The college image: competitiveness, concern for students, spirit, permissiveness, snobbishness, flexibility of the curriculum, and emphasis on social activities.

The design of the ACE research program incorporates (a) Inputs: the talent, skills, and other potentials students bring with them to the college. (b) The college environment: aspects of the institution that are capable of

affecting the student, like policies, practices, curriculum, physical plant, and teaching methods. (c) Outputs: the operational manifestations of educational objectives—the skills, attitudes, and behaviors of students that are influenced.

The book by Astin & Panos (8) is the third in a series reporting on the ACE program of research on student development which began in the fall of 1961 with an assessment of the freshmen classes entering a national sample of 246 institutions in the fall of 1961. Data provided by the 127,000 freshmen at the time were analyzed and reported in the book *Who Goes Where to College* (7). A 1-year follow-up study of 30,000 of these students was carried out in the summer of 1962 and reported in the second book, *The College Environment* (6). The current report was based on a follow-up study carried out in the summer of 1965, 4 years after matriculation, and involved some 36,000 of the original students.

A major purpose of the study was to assess the significance of institutional diversity by comparing the effects of different college environments on undergraduate students' educational aspirations and career plans. The specific objective was to identify institutional characteristics and educational practices that affect the students' chances of completing college, going on to graduate school, or pursuing a career in a particular field. Astin & Panos conclude that to compare the effects of the different institutions it is essential to control differential student inputs. They have also shown that while institutions do appear to have differential effects on their students, the magnitude of these effects is small compared to the effects of the student inputs. Astin & Panos studied differential college impact by using three types of data: input or pretest information, showing the status of the student when he first enters the college; output or criterion information, showing the status of the student after he has been exposed to the environment of the institution; and environmental information, describing the institutional characteristics to which he is exposed.

Their findings indicated that about one student in ten changed his major field during the senior year and about one in four changed the long-term plans for his career. Apparently, for many undergraduate students the question of career choices is still largely unsettled during the last year in college.

Perhaps the most important finding about how the environment affects the students' educational and vocational plans is that the student's career choice changes to conform more and more to the dominant modal choice of his peers as he progresses through the undergraduate years. In other words, the student is more likely to maintain his initial choice of a given career or to shift from some other choice into that career if a relatively high percentage of his fellow students also choose the same career. Students in universities seem to drop out of college more than students in liberal arts colleges, and they lower their educational aspirations more frequently. Panos & As-

tin hypothesize that this may be attributed, at least in part, to a relative lack of faculty and student involvement in class, to the rather impersonal relationship between instructor and students, and to a relative lack of concern for the individual students in most universities.

Liberal arts colleges increase the student's interest in attending graduate school and obtaining a PhD degree. Regarding the type of control, Astin & Panos found that attending a public institution increases the student's chances of dropping out and decreases the interest in attending a graduate school. Public institutions also tend to shift students out of the arts and humanities and social sciences into business and education.

One of the most unexpected findings of the study was that individual student achievement (measured by the three area tests of the Graduate Record Examination administered during the senior year in college) is neither improved nor impaired by the intellectual level of classmates nor by the level of academic competitiveness nor by the financial resources of the institution. Similarly, no evidence was obtained to support the common belief that the bright student benefits more than the less able student from exposure to these traditional features of high-quality institutions. In general, differences among students and their achievements at the senior level are much more closely linked to variations in ability that existed prior to the student's entrance to college than to any characteristic of the undergraduate institution. They also found that the cohesiveness of the peer environment has a pronounced positive effect on persistence in college. By contrast, colleges with relatively incohesive environments have dropout rates that are much higher than predicted from their student inputs.

Astin & Panos suggest that their findings call for a re-examination of some of our traditional notions about institutional quality or excellence. The student's later intellectual achievement, educational plans, and career choice are related much more to his characteristics and plans at the time of matriculation than on his choice of undergraduate institution. The implication here is not that his choice of a college is an unimportant factor in his development but rather that the college environment is of little relative importance in comparison to his initial input characteristics in determining the student outcomes considered in this study. Future research should be conducted to determine whether the "progressive conformity" effect for career plans holds true for other student characteristics, including values, beliefs, and personal trends and behavior patterns. A related hypothesis which should be subjected to further empirical tests concerns the significance of environmental homogeneity. Briefly, findings by Astin & Panos suggest that the potential of the college environment for effecting significant change in the student is a direct function of its degree of homogeneity, a position also held by Newcomb & Feldman. Since most of the environmental effects in the 246 institutions appeared to be mediated through the peer environment rather than the classroom, administration, or physical environment, further

study of the nature and influence of undergraduate peer groups is clearly indicated.

Chickering (26), in reporting a 5-year study of student characteristics, institutional characteristics, attrition, and student development in 13 small colleges, discussed seven major dimensions of student development that occur during college years: intellectual competence; management of emotions; development of autonomy, identity, and interpersonal relationships; purpose; and integrity. He hypothesized that college variables influencing student development along the seven dimensions are: institutional size; clarity of objectives and internal consistencies; curriculum, teaching, and evaluation; residence hall arrangements; faculty and administration; friends, groups, and student culture.

Heath (45) studied, by the participant observer method, student development at Haverford College for 4 years. His purposes were to illustrate how a model can be used to order the developmental process, to illuminate potential types of maturing effects, to explore in detail the relationship between the institution and the growth of its members, and to suggest new hypotheses about healthy growth of students.

In order to study the personality changes of the students, each person was tested in the first week of his freshman year and then again at the end of the freshman or senior year. Results were discussed with each student personally, after which time intensive, regular interviews were used to identify the determinants of the student's development in college. Each student was tested and interviewed for a total of 11 to 12 hours.

Some of the tests administered were the Allport-Vernon-Lindzey, *A Study of Values*, the *Strong Vocational Interest Blank*, the *MMPI*, and two questionnaires that Heath developed, the *Self Image Questionnaire* (SIQ) and the *Perceived Self Questionnaire* (PSQ).

A two-stage research program was designed to study the development of three randomly selected groups of students at different points in their college careers and identify the determinants of that development. Studies of alumni to discover whether the changes that occurred in college persisted were also reported.

Heath described the maturation of students. According to him, freshmen, shocked by the demands of college, become unstable and disorganized, more aware of their own inadequacies, and question their own competence and values. The students then find their greatest support from peer relationships and become preoccupied with personal relationships, especially sexual ones. The students eventually turn to the development of intellectual skills and become more stable and autonomous.

Heath argues that the "sophomore slump" is a consequence of the student's argument with himself about what direction his life is going to take. The student's self-concept stabilizes when he decides on a vocation, but he doesn't really have a firm sense of his own worth until he tests his compe-

tence as an alumnus. During the student's junior year much consolidation occurs, and in the senior year the student begins to stabilize heterosexual relationships, his values become more integrated, and he is eager to test himself. At graduation the student still has to complete some of the following developmental tasks, according to Heath: stabilize his heterosexual relationships, integrate and stabilize his values and self-concept, and continue the development of increasing his autonomy. The effect of Haverford College, Heath concludes, is that it provided a model of values that was the core around which the students' growing integrity as persons was organized.

Heath's theory is that development is an organismic process that must be studied over a time span sufficient to illuminate the complexity of the interactive dimensions that define maturing.

He concludes that change is mediated primarily by the quality of one's personal relationships with others and the expectations that others have of the type of person one should become. He says that a college is liberating in its educational program if the students are educable, if there is a communal character to the college, and if there is an internal coherency of purposes at the college.

Progress is being made, albeit slowly, in conceptualizing student development in the context of higher education. Counseling psychologists have the opportunity and responsibility to contribute to the complex task.

VOCATIONAL BEHAVIOR AND DEVELOPMENT

The reviewers have not attempted to review critically all literature on vocational behavior, but have selected several articles of particular interest from the student development viewpoint.

Although vocational behavior theorists seem to view a person's vocational behavior to exist in an environmental context and hint at personological-environmental interaction, they, in the main, have not developed research designs that explicate the result of that interaction.

Vocational development theorists agree in conceptualizing choice making as a dynamic and complex process with the growth of the individual of central concern. Thus, Super (80) views occupational choice as implementing the individual's self-concept, and Wheeler & Carnes (93) believe that the occupational choice process reflects attempts at self-actualization. Job activities provide various modes for expressing and controlling one's impulses and thus the organization of one's adaptive approach to the world, according to Bordin et al (16). Beall's (14) notions of the impossible fantasy and improbable choice represent unsuccessful ambivalent identification on the one hand and a choice in which the impulse expression allowed by the occupation is too frightening to the individual on the other. Holland (49) believes that an individual will find a work setting congruent with his life's style. The work situation will provide for the satisfaction of needs associ-

ated with that life style. Roe (74) postulates that the individual, because of his family environment, develops basic attitudes, interest, and capacities which will be given expression in the general pattern of the adult's life in his personal relations, in his emotional reactions, in his activities, and in his vocational choice.

Hershenson (46) has presented a life-stage vocational development system, i.e. he organized vocational development theories within a life-stage framework. Thus, he attempted to locate existing vocational development theories within a consistent theoretical framework. He utilized Freud's life stages extending from the oral stage to that of maturity.

The vocational development process, according to Hershenson, may be considered as a series of five sequential stages: social-amniotic, self-differentiation, competence, independence, and commitment. These stages are arbitrary, for the focal constructs in life-stage theories tend to determine the number and definition of their stages.

Hershenson makes the following assumptions: (a) these are sequential stages, each one sets the limiting conditions for the subsequent ones; (b) both growth and deficit motivation exist throughout the life span. As deficits are overcome, further growth is promoted, and as growth presses forward, there is increased pressure to make up deficits. A key concept of Hershenson involves that of energy utilization (both physical and psychic), and he has five stages of energy utilization corresponding to the five vocational development stages listed. His life stages extend as far as the Freud stage of maturity which is coterminous with Hershenson's vocational development stage of commitment. He does not make provision for old age, decline, and retirement as have other theorists such as Super and Havighurst, and it would be desirable to include also the developmental stage of youth as proposed by Keniston. A concern for these segments of the life span seems important for vocational development theory. Hershenson has interrelated the theorizing of life-stage developmental theorists and of vocational development theorists to some extent. The field of vocational behavior needs synthesizing by an eclectic theorist. It is unfortunate that Zytowski (96) did not attempt that difficult and demanding task in his book of readings, *Vocational Behavior*.

How does an individual choose which of several possible lines of development he will follow and which he will cease to consider? The basic proposition of Tyler et al (87) is that the environment with which a human being copes is a universe of possibilities. As society becomes more complex and as affluence, mobility, and communication increase, the direction each individual's life takes is determined increasingly by his own choices and decreasingly by external circumstances. Tyler et al suggest that the anomie and identity confusion common among teenagers from privileged sectors of American society have come about because young persons have not been

prepared to make responsible choices at the time they are confronted with the need to make decisions.

Tyler et al compared three similarly selected samples of early adolescents from the United States, the Netherlands, and India using the choice-pattern procedure (86). This is a sorting technique designed to reveal the nature of the concepts a person uses to select from the possibilities available to him of courses of action that determine the direction of his later development. Numerical scores and style and content aspects of the sorting of occupational titles were compared for the three samples. Results indicated that concepts for thinking about occupations and the self were most numerous and highly differentiated in the American group, least numerous and differentiated in the Indian group. All three groups emphasized intrinsic rather than extrinsic aspects of the work tasks. A general hypothesis growing out of the findings is that external structuring of the environment and internal structuring of possibilities are inversely related.

Thus, in India where much of a person's vocational planning is done by his family, individuals do not develop highly differentiated concepts about kinds of jobs. In the Netherlands, where external structure is provided by the secondary school system, individuals consider and form concepts mainly about the occupations within an appropriately narrowed range. In the United States, where all young people attend the same schools and where family expectations tend to take the form of requiring that the person "make something of himself," the individual produces the most highly differentiated conceptual structures.

Occupation can be viewed as a means of self-actualization, and a person may use his vocational choice as a means of becoming the kind of person he wants to be, of promoting his values in reaching his goals according to Wheeler & Carnes (93). Male college students were provided a measure of self-concept modified from Blocher's Descriptive Check List and were instructed to respond in terms of self and ideal self and in terms of the characteristics of individuals in their probable occupation and in their ideal occupation. The subjects saw their ideal selves and typical people in their probable and ideal vocational choice occupations as quite congruent. Yet all three were different from their present concepts of themselves. Both occupational stereotypes were more congruent with their ideal selves than with their real selves. Thus, occupational choice may be viewed as actualization of the self and not limited to implementation. Bordin et al (16) presented a new approach to the analysis of jobs which identified the various avenues provided by job activities for expressing and controlling one's impulses. This approach to vocational behavior is one that conceptualizes occupational choice as a dimension of personality development rather than a process of aptitude matching. Beall (14), following Bordin, also viewed occupational choice as one aspect of personality development within the frame-

work of psychoanalytic theory. The basic assumptions are as follows: 1. Occupations can be described in terms of personality dimensions. 2. Different occupations provide different opportunities for the expression of impulses, the utilization of defenses, and the organization of the individual's adaptive approach to the world. 3. An adaptive occupational choice matches occupational skills, individual aptitudes, and the opportunities the occupation offers for the expression of impulse, with the modes of impulse gratification the individual comfortably utilizes. Reflecting this theoretical approach, Beall describes two vocational difficulties of individuals.

One vocational difficulty preserves "an impossible fantasy" which functions to maintain a defensive ambivalent identification in an encapsulated fantasy. Another type of vocational difficulty occurs when the vocational choice involves an "improbable choice" (a defensive solution) that is either so far from a forbidden impulse that no gratification is allowed or so close to the impulse that the danger of expression is constantly generating anxiety and interfering with gratification. The counselor's role, especially his diagnostic task, in assessing the client's impulse defense organization is stressed by Beall.

The research of Faunce (37), Watley (92), and Helen Astin (9) has added to our knowledge of the relationship of personal characteristics to educational-vocational behavior. In the Faunce study, noncollege graduates more than graduates were troubled by personal developmental problems which interfered with the effective use of their abilities. In contrast, college graduates had characteristics which enhanced and promoted effective motivation and study and helped the achievement of satisfactory interpersonal relationships and effective maturity. Thus, personality characteristics of freshmen women and their adjustment influenced the degree to which the students were able to make use of their own potential. Faunce's research also reinforced the notion that regardless of how much ability a person has, that person will be unable to achieve consistently unless his ability is accompanied by appropriate interests. Watley's study indicated that when ability is not a factor (e.g. when one is considering students only of high ability), then sociocultural, attitudinal, and personality factors enter into determining achievement in school. Helen Astin found that measured and expressed interests were the only important predictors of twelfth-grade career plans of ninth-grade boys and girls. All three investigators studied only high ability persons so that the influence of nonintellective variables on achievement and career planning was not masked by variations in ability.

Personality characteristics and vocational interests of 723 gifted college freshman women who graduated and 526 who did not graduate were compared by Faunce (37). MMPI results revealed that nongraduates had less insight into their own personality structures, greater difficulty in interpersonal relationships, more problems with impulse control, and greater inner tensions. Graduates were more insightful, conventional, temperate, modest,

self-confident, had better ego strength and psychological integration, and were relatively free from tension.

Nongraduate women evidenced a lack of academic drive. Also apparent were study difficulties, including an inability to concentrate and poor study habits, as well as a tendency to be tense on examinations and worry over scholastic failures. Graduate women on the other hand portrayed good general adjustment; they were more conventional, temperate, and modest. They possessed a self-concept which included personal reserve and self-enhancement.

Watley (92) studied highly gifted students 7 to 8 years after they entered college to determine variability in career progress and to identify variables contributing to differential achievement. National merit scholars of both sexes differed markedly in the level of education achieved. This was not due to unequal scholastic ability or to differential high school achievement, but to factors relating to family background and scholar personality characteristics.

Males of high ability who did not achieve the baccalaureate degree had relatively more than their share of emotional adjustment problems. Evidence of this was found on the *California Psychological Inventory* (CPI) on self-ratings and on the fact that more of them received psychotherapy. Their earliest college years may have been frustrating because almost two-thirds of them began college with doctoral level ambitions. Until they entered college, few of them had experienced difficulty with academic achievement. As inferred from father's occupational status, low family income, lack of parental involvement in educationally oriented activities, and the relative lack of instructional materials in the home, there appeared to be a relative lack of emphasis on learning in the home.

Male Bachelor of Arts or Bachelor of Science degree recipients were relatively well adjusted. The CPI results revealed that they were rather easy-going individuals who lacked the drive and ambition necessary to achieve to the extent of their capacity. Their family background variables indicated there did not appear to be a strong emphasis on education in the home.

Those who attempted some graduate work also had family characteristics that placed less value on educational and intellectual pursuits as compared to the families of the most educationally advanced scholars. However, they were ambitious and had high aspirations. The most important goal of most of them was to find an interesting and worthwhile role in life. Almost all had career commitments and a high percentage expressed satisfaction with their career decisions.

The males who earned doctoral or professional degrees came from intellectually stimulating home backgrounds. Their parents were the best educated of the male groups. Almost two-thirds of their fathers worked in the professions or owned their own businesses. These high-achieving scholars

developed patterns of behavior that contributed to their academic and intellectual excellence. They had high vocational aspirations and were industrious, persevering, and motivated to excel.

The women in Watley's study who did not attain a baccalaureate degree also came from home and socioeconomic backgrounds which influenced their reaction to the collegiate experience. Their home atmospheres emphasized practical aspects more than intellectual ones. They were not strongly career oriented.

The women receiving only a Bachelor of Arts or a Bachelor of Science degree were family oriented. Many of them were married and were seeking the responsibilities of motherhood at the time of the follow-up.

Those women who engaged in some graduate work when compared with other groups, males included, came from homes which provided a superior climate for pursuing intellectual excellence. About 60 percent of both parents were college graduates and were actively involved in scholarly activities. Their homes were well stocked with educational and learning materials.

Helen Astin (9) utilized the Project Talent data bank to predict the career plans of 817 female high school seniors from their personal characteristics as ninth-graders and from selected environmental characteristics of their high schools. Seniors choosing different careers could be differentiated in terms of interests, career plans, and aptitudes as measured during the ninth grade. The discriminatory power of a ninth-grade test battery was improved by adding selected environmental characteristics of the high school attended by subjects. However, the environmental characteristics were primarily a reflection of the size of school.

Twelfth-grade girls who planned college careers either in sciences or in teaching were differentiated from girls who planned either to become housewives or to do office work by these ninth-grade measures: higher socioeconomic status of family, greater mathematical ability, more frequent choices of a college preparatory curriculum, and stronger measured interest in physical sciences. Girls who attended larger high schools were more likely to plan careers in the professions and sciences than those attending smaller high schools.

Crites & Semler's (32) research described the vocational development of adolescents by a hierarchical construct in which adjustment is a general dimension and educational achievement and vocational maturity are more specific dimensions. A factor analysis might have yielded a further delineation of the relationships in their data. Other researchers who showed ingenuity in applying constructs to their data were Baruch, Segal & Handrick (13). They defined conceptual schema into which work and family roles could be organized and used the following labels: purposeful breadwinner, oppressed breadwinner, role polarizer, conflicted, and happy harmonizer.

Vriend (90) demonstrated that high school students could be accelerated

in their vocational development by a 2-year program of structured vocationally related activities. His subjects were innercity school students.

It seems from the literature reviewed that vocational development theorists are groping toward a framework within which personal characteristics interact dynamically. However, research which simultaneously studies dynamic personal characteristics and their interaction with dynamic environmental variables, including experimentally applied treatments, is still to be conceptualized and conducted. Not until such research is done will vocational behavior theory advance significantly.

COUNSELOR TRAINING

In the time period covered by this review there was relatively little attention given to research on training of professional counselors. Noteworthy was increasing attention to the use of peers or less than professionally trained persons to offer conditions facilitative of growth in others. A concomitant of this trend should be the training of counselors as supervisors of and consultants to the subprofessionals. There is some literature concerned with supervision of counselor trainees and the use of group processes. Altucher (1) indicated that the goal of counseling supervision is to help the trainee remain open to his own experiences. Altucher assumes that learning to be a counselor is both an emotional and an intellectual experience with the emotional part most crucial. Betz (15) reported that counselors receiving "affective" group counseling significantly increased in their ability to respond to counselees' affect but did not show corresponding changes in "degree of lead" or variability of response patterns. Betz concluded that teaching response to affect in group counseling carries over to individual counseling behavior.

Hurst & Fenner (52) found that a marathon group counseling session with counselor trainees provided a direct sample of some of the behaviors usually associated with counseling competence: openness, acceptance, empathy, sensitivity, and effectiveness in communication. The use of a group session early in training would provide diagnostic information which could be helpful in the training program.

Pierce & Schauble (72) reported that counseling interns having supervisors who functioned at high levels of empathy, regard, genuineness, and concreteness (Carkhuff model) changed positively, i.e. changed in the direction of higher levels of functioning in these behaviors. Interns who had low level functioning supervisors on these dimensions did not change.

Miller (66) and Reddy (73) demonstrated that reinforcement techniques can be used to teach emphatic and understanding responses.

Carkhuff, Kratochvil & Friel (25) found that clinical psychology trainees did not change in their therapist functioning from beginning to advanced stages of training. Ratings of nine clinical professors suggested that in general the trainees moved in the direction of the level of functioning of

their professors. Graduate training did increase ability to make discriminations of the level of conditions offered by other counselors in taped excerpts. The results of this study were used by Carkhuff (21) to question the necessity for professional training for persons performing the helping function. Carkhuff listed six advantages the person of the lay counselor and the help he offers have over that provided by a professional. As Schmidt (76) indicates in his reaction to the Carkhuff article, there is a challenge as well as a threat to professional help-giving as a result of the demonstrated effectiveness of less than professionally trained help-givers, the increasingly heavy demand in our society for helpers, and the shortage of those helpers professionally trained.

Shapiro & Voog (78) tested the possibility that certain individuals have therapeutic qualities which enable them without training to facilitate others' functioning. They studied 56 male freshmen who were rated on therapeutic functioning by their roommates, by students next door, and by dormitory counselors. Ratings by roommates and students next door were positively related to grades earned by the roommates of subjects. It was concluded that those qualities which differentiate helpful counselors have similar effects on others. A replication of this study with treatments experimentally applied and with predictions of the direction of impact and amount of it on persons to be influenced would help verify that certain persons have a facilitative impact on others.

As indicated by Keith-Spiegel & Spiegel (54), persons at levels of intelligence compatible with that of patients were perceived by the patients as being helpful to them. Martin, Carkhuff & Berenson (64), however, found that the levels of facilitative conditions offered by a professional counselor were significantly higher than those offered by the "best available friend" to college students who were set to discuss problems they might have. Whether or not "best available friends" could be quickly trained to offer, under supervision, the same level of facilitative conditions is not known. Gordon (43) had earlier made a plea for training of subprofessionals to perform work now being done by fully qualified counseling and clinical psychologists.

It is clear that there will be an increasing employment of subprofessionals to provide student counseling services. Counseling psychologists will need to give increasing attention to training and supervision of subprofessionals and provide consultation to them. Hopefully, counseling research will provide information helpful to the subprofessionals and their trainers and supervisors.

COUNSELING RESEARCH METHODS

Counseling attempts to facilitate change in the individuals it serves. It requires a set of operations or procedures which can be applied to assist a person to acquire modes of performance which, for his or for society's purposes, are deemed desirable. Because counseling is concerned with behavior

change, knowledge of methods for assisting individuals to achieve the changes they seek is essential to the counselor. Validation of existing methods and development of new methods depend on research. To what extent does the literature of the counseling field report research on behavior change methods?

Brown (17) reported a survey of articles published over a 7-year period in *The Personnel and Guidance Journal*. Empirical studies made up 55 percent of the articles. The studies included surveys of the characteristics of particular groups of individuals, program surveys, comparisons of two or more groups, test development and evaluation, correlational studies (relating several scales or variables), and prediction studies. Empirical studies of this kind can provide descriptive data, but they do not establish causal relationships and hence do not contribute much to theory development. Only about 6 percent of the articles reported were true or quasi-experiments.

Brown found that articles descriptive of personnel programs were second most frequent. Approximately one article in eight (12.5 percent) was theoretical or conceptual, and counseling theory articles were very few (5.3 percent). Thoresen (83) surveyed the articles from a 2-year period in the *Journal of Counseling Psychology*. He found in the 1967 and 1968 volumes that approximately one-third of all articles were not research reports. Descriptive-correlational studies were reported in over 75 percent of the research articles. The remaining 25 percent of research articles reported experimental studies. No articles reported controlled observation and change of individual behaviors.

In the *Psychological Abstracts* of 1968 and 1969, 270 articles were identified by this review's authors as assignable to categories relevant to college-level counseling. Eighty-five percent of these articles and reports described nonexperimental inquiries. In nearly 40 percent of these inquiries academic achievement was studied in relationship to some characteristic of students: personality variables; cognitive styles; demographic data such as birth order, sex, and socio-economic variables; and self-descriptions. About 12 percent of the correlational studies related academic achievement to factors in the college environment, such as support services, advising, and tutoring. Other nonexperimental reports concerned descriptions of performances by special groups, e.g. women, minority groups, underachievers or gifted students in particular programs, curriculums or schools (16 percent), and general student personnel program descriptions and commentaries (20 percent). Stress, anxiety, and dropping out of college appeared in about 20 percent of the correlational and descriptive articles. The predominant theme emerging from this summary of articles is the study of the relationship between personal characteristics of students and the students' academic success, failure, or retention in the college. Most studies related only two factors. Grade-point averages constituted the most frequent standard for academic achievement. Studies relating aspects of the learning environment or

academic community and academic achievement were minor in number compared to those relating personological factors of the student to his academic performance.

About 15 percent of the 270 articles could be classified as experimental. A third of these concerned the systematic desensitization of test anxiety. Another third compared different counseling methods, usually in group settings. The remainder of the experimental articles examined the effects of different test information feedback procedures on subsequent learning, the effects of different study strategies in reading and study skills performance, and the effects of hypno- and chemotherapy on academic performance.

Counseling methods must be based upon knowledge of causal relationships. That is, what can the counselor *do* to bring about the changes sought by the client? Descriptions of student characteristics and correlational data will not give the counselor the behavior change methods he needs. Brown's and Thoresen's reviews of the two major journals in the counseling profession, as well as the overview of abstracts from *Psychological Abstracts*, strongly suggest that the counseling literature is not providing the counselor with the experimentally based information he needs in order to be effective. Further, according to Thoresen's review, a sizable proportion of the experimental studies have sufficient defects in design and controls to seriously restrict their utility for developing counseling methodology. Even so, because experimental studies are far from the modal research activity in counseling, it appears that the counseling literature is producing little that the counselor needs to accomplish his mission to facilitate changes desired by his client.

Segal (77) hoped counseling psychologists would seek alternatives to traditional research and theorizing by attending more closely to the often changeable contextual variables in a developmental setting. Subsequent reviewers, such as Thoresen (83) and Hosford & Briskin (51), called for increased use of multivariate research designs in counseling experiments. Research designs which involve multiple independent variables, dependent variables, and outcome criteria are more likely, they aver, to reconstruct the complexities of field applications in counseling. Single treatment-single control-single criterion experiments not only oversimplify counseling interactions, but are out of date considering what we know about research design. Counselors espousing humanistic and phenomenological viewpoints frequently eschew research activities and research literature as being unrealistic and unrepresentative of human enterprise and the intricacies of counseling relationships. Simplistic research designs used in counselor research give credence to their arguments.

To some degree Segal's urgings to counselors to apply various research designs in their investigations have been realized. Bandura (11) reported studies employing intrasubject replications in which behavior was successively eliminated, reinstated, and again eliminated by systematic variation

of reinforcement contingencies. Cross-sectional studies have been reported by researchers such as Krumboltz et al (56) in which simulated occupational experiences were employed to determine effects upon vocational interests of students in different secondary school settings.

A great amount of correlational data exists in counseling research. Although some of these data may be useful for hypothesis generation, they are insufficient for theory development, establishment of causal relationships in counseling operations, and development of effective counseling methods for certain clients in particular settings. The research accomplished to date has not permitted much generalization of findings. What is needed are research strategies which will combine the evidences concerning characteristics of the client or learner with manipulatable characteristics of the counseling or learning setting. Results of such research should not be expected to demonstrate one best method or theory, but would rather suggest optimal environmental and learning conditions for clients with particular characteristics or initial abilities. The aim of such research should be to identify personal characteristics and environmental variables which interact in modes which are significant to the particular counseling problem. Multivariate methods have been shown to be useful in establishing such differential relationships. Still an additional method may have utility for the counseling researcher. This method is the aptitude-treatment interaction (ATI) procedure.

Cronbach & Snow (33) offer a description of the ATI model as it applies to their research and that of others. They consider ATI studies, based upon regression statistics, to be a significant alternative to the experimental and correlational methods most frequently applied in current behavioral science research.

ATI studies have been performed primarily in educational contexts; given learners of certain characteristics, what instructional means work best toward achieving a certain level of learner proficiency? The ATI model seems to have relevance for counseling research not only in attempts to identify the relative effects of particular counseling techniques with clients of varying characteristics, but in the decision-making required for the placement and recommendation of certain students for appropriate academic programs. ATI methods, as correlational methods, cannot yield information on cause-effect relationships. Nevertheless, such interactional studies can aid in identifying critical relationships between the individual and his environment, in theory building, and in setting the stage for experimentation.

The wealth of individual differences among students, variability in situational treatment and temporal conditions, and complexity of expressed problems cannot be ignored; variability must be accounted for to the best of the researcher's ability and his results so qualified. Simplistic experimentation, such as comparing "counseling" to "no counseling" control groups, or a par-

ticular method to no treatment at all, and correlational studies fail to yield information about simultaneous operation of significant factors in the counseling setting. Appropriately applied research designs which supply interactional data can reduce the tendency to seek the "one best method for all time" and supply the kinds of information counselors sorely need.

COUNSELING PROCESS RESEARCH

The paucity in the counseling literature of experimental research on the counseling process necessarily restricted the scope of this review.

The writing and research of Carkhuff was extensive and noteworthy during the period of time covered by the review, and his work stimulated others. Carkhuff and his colleagues studied high and low functioning counselors and clients, high and low functioning on the processes involving interpersonal conditions of empathy, positive regard, genuineness, and self-disclosure (Carkhuff 20). The evidence is that there is a relationship between the conditions offered by the counselor through his behavior and the behavior of the client. Anderson (4) found that a therapist's ability to confront his clients with discrepancies in their ways of viewing reality is associated with increased client self-exploration following the confrontation. The therapist whose confrontations led to increased client self-exploration were all found to be highly rated by expert judges on the facilitative dimensions of empathy, positive regard, genuineness, and self-disclosure. In her followup research Anderson (3) found that at high therapist levels of functioning, confrontations were followed by increased client self-exploration to a significantly greater degree than was the case under low therapist conditions.

Holder, Carkhuff & Berenson (48) found that the depth of self-exploration of low functioning clients was found to be significantly related to the level of interpersonal conditions offered by a counselor. The exploration of the high functioning clients continued to be independent of the level of conditions offered by the counselor and was significantly higher than that of the low functioning clients. Carkhuff & Alexik (23) found that low functioning counselors were manipulated by a counselee who explored herself deeply during the initial and last one-third of the interview but experimentally reduced her depth of self-exploration during the middle one-third of the interview by discussing irrelevant and impersonal material. Counselors who had been functioning at high levels of facilitative conditions were not manipulated by the lowering of self-exploration and continued to function at high levels. There was a tendency for the highest functioning counselors to increase the levels of conditions which they offered during the experimental period. The obvious limitation to this study is that only one client subject was utilized.

Carkhuff (22) also found that level of communication and discrimination were related to counselor experience or training and the affect and content of client statements. Four-hundred-forty subjects at seven different lev-

els of helping experience and training responded to 16 client stimulus expressions in which client affect (depression, anger, elation) and content (social, vocational, child rearing, marital, and confrontation of helper) were crossed and rated 64 counselor responses (four to each client expression) in which high and low levels of counselor facilitation and action orientation were crossed. Those counselors with the higher levels of helping experience or training demonstrated higher levels of communication and discrimination. The reader is referred to the book by Carkhuff & Berenson (24) for a further development of their theorizing and for abstracts of research carried out under their direction.

Keith-Spiegel & Spiegel (54) and Bare (12) found relationships between characteristics of helping persons and the effectiveness of the helping relationship. Keith-Spiegel & Spiegel found that patients discharged from a mental hospital perceived as most helpful to them those staff members and patients whose intelligence level was compatible with theirs. The more intelligent patient tended to designate the psychiatrist or psychologist as most helpful, while the less intelligent patients tended to choose aides and other patients as most helpful.

Bare reported that counselor personality characteristics and counselor-client similarity on personality measures were related to the counselors' general effectiveness, empathy, and facilitation of a close relationship. Couselees and counselors agreed that these three criteria of counseling success are best achieved when counselor characteristics include high original thinking, high vigor, low achievement needs, and low order needs, and when counselors are unlike their clients on such variables as original thinking, vigor, and responsibility.

Several investigators studied the helping relationship through factor analyzing an evaluation inventory, counselor communications, and a relationship inventory. Haase & Miller (44) factor analyzed the Counseling Evaluation Inventory (CEI). Zimmer & Park (95) factor analyzed counselor responses in two counseling sessions, and Mills & Zytowski (67) factor analyzed the Barrett-Lennard Relationship Inventory.

In their factor analysis of the Counseling Evaluation Inventory (CEI) on secondary school students, Linden, Stone & Shertzer (61) had labeled factors counselor comfort, counseling climate, and client satisfaction. Haase & Miller administered the inventory to college students and defined a general factor which combined counselor comfort and counseling climate. Thus two factors were defined, counseling climate and client satisfaction. Zimmer & Park (95) factor analyzed counselor responses in two counseling sessions with one client. These responses were rated on a warm-cold continuum by 77 students enrolled in guidance courses. Ten factors were extracted for the first session and eight factors for the second. The factors identified had a strong Rogerian flavor which may be a reflection of the students' previous training rather than anything intrinsic in the interview. The Zimmer &

Park technique is noteworthy but because of the small number of subjects, the single client, and student expectancies, further research must be completed before attention is paid to the factors defined.

Mills & Zytowski (67) factor analyzed two forms of the Barrett-Lennard Relationship Inventory administered to undergraduate female college students. They also reanalyzed data reported by Barrett-Lennard, which he obtained from clients in counseling and from their therapists. Three components were isolated: a general component, a component reflecting a reciprocal relationship between Unconditionality and Level of Regard, and a component tentatively identified as a relationship distorting component.

Factor analytic research such as that of Zimmer & Park and Mills & Zytowski is handicapped by the small number of available cases and the resulting unreliability. The research by Thompson & Zimmerman (82) illustrates another of the difficulties in doing research in counseling. Of the 315 clients studied, 146 or 45 percent came to the counseling center for only one interview. The remainder were seen from 2 to 25 times with a mean of 4.28 interviews. The purpose of the Thompson & Zimmerman research was to compare the goals of counseling as perceived by the clients and by their counselors. It is difficult to believe that there was time in one interview for the counselor and client to become well enough acquainted to participate in this kind of research. Thompson & Zimmerman reported a marked discrepancy between the goals of clients and counselors and instability of both sets of goals.

The research of Ewing & Gilbert (36), Goodstein (42), and Gilbreath (40) also raises the question of stability. All of these investigators, using improvement in grade-point average as the criterion for counseling effectiveness, found increases in criterion scores immediately following counseling. However, follow-up studies by all investigators indicated that the positive results did not persist. This was true of the follow-up study conducted by Ewing & Gilbert the semester following counseling, and of Gilbreath 3 months following the experiment. Goodstein followed up his subjects 5 years after the counseling experience. As Goodstein indicates, his results cast doubt upon the usefulness of brief traditional, educational, vocational counseling for dealing with academic problems of freshmen underachievers.

BEHAVIOR MODIFICATION, COUNSELING

The three main behavioral approaches are those deriving from classical conditioning, instrumental conditioning, and social learning theory as presented by Rotter and Bandura and directed toward counseling by Krumboltz & Thoreson (57). Because of space limitations, the review authors have not attempted to review the contemporary status of behavioral counseling. Instead the reader is referred to a review by Hosford (50).

Perhaps the major contribution to date of the behavioral approach to

counseling is the requirement of specifying individual goals or desired outcomes of client and counselor in observable terms. In addition, the approach has provided counselors with a growing collection of methods for achieving behavior change, and with a research base for continuing development of counseling methods. Notable recent collections of methods appear in texts by Mehrabian (65), who presented a general schema for behavior change; Bandura (11), who described the breadth of behavior modification research with particular emphasis upon modeling and social learning; Krumboltz & Thoresen (57), who illustrated the application of behavior modification methods in educational settings; Ullmann & Krasner (88), who described the application of behavior modification techniques to psychologically deviant or abnormal populations; and Ayllon & Azrin (10), who presented research on operant programs within institutional settings.

CHALLENGES TO COUNSELING

Coons (30) discussed five prominent developmental tasks of the college age group: 1. shift in nature of relationships with parents; 2. resolution of personal sexual identity; 3. creation of a value system; 4. development of capacity for true human intimacy; and 5. choice of life work. These tasks overlap with those presented by Erikson (35). The need for counselors to know how students can effectively deal with these tasks is obvious. In recognition of the need to help students develop, college counseling centers are gradually giving up their passive and reactive roles isolated from the mainstream of student life and are changing into student growth and development centers with the focus on the educational and developmental tasks in addition to the traditional remedial functions (Levy 60, Foulds & Guinan 38, Lipsman 63). Carey (19) stated that student protests indicate that counseling has been ineffective in that the individual student's needs have not been met satisfactorily. He said that the counselor should be the listener of students and learn of their attitudes, values, and needs before the students take indirect ways through demonstrations and violence to indicate what their needs are. Such listening will necessitate the counselor moving out of his office to where the action is and feeding the information gained into proper administrative channels so that change can occur. Lawton (59) and Calia (18) have also emphasized the need to move out. Crisis intervention (Garner 39) and the campus crisis center (Trucker, Megenity & Vigil 85) are techniques being used to reach students who ordinarily would not be reached by a counseling center.

It is possible that the counseling center of the future will be staffed in a large part by professionals trained at less than the PhD level who will be able to work comfortably in the student milieu. As indicated above, Carkhuff (21) and Gordon (43), among others, have suggested the use of non-professionally trained, or lay people, in the therapeutic or counseling process. Shapiro & Voog (78) demonstrated that male freshman roommates

who were described as inherently helpful could be helpful to their roommates. Wolf (94) demonstrated that indigenous subprofessionals can promote growth of college freshmen in group discussion settings. Members of groups led by psychology graduate students improved only slightly more than those led by dormitory advisors. Counseling centers could take the lead in involving selected undergraduates in working with others in a helping role. The professional counselor can serve as a consultant and supervisor to the peer counselors and thereby multiply his effectiveness.

CULTURES AND COUNTER-CULTURES

University freshmen and sophomores are increasingly participating in campus protests. Incoming freshmen arrive on campus expecting to engage in campus protests as a part of their university experience in much the way that earlier student generations expected to be a part of the Greek system. Some of these freshmen are already alienated. Alienated persons (i.e. persons at odds with the predominant culture) have established counter-cultures with value structures which give them significance (Roszak 75). Although these counter-cultures vary in the scope of extent of their discrepancies with the prevailing culture, their distinguishing characteristic is that their values are in conflict with those of the predominant culture.

A university campus cultural milieu reflects many of the cultures extant in the society. In fact, one can argue that one aspect of the excellence of a university is the cultural diversity of the students and faculty. In a large university, new groups can be formed by persons who become aware of their mutually held values—if such persons exist in critical enough numbers and if they find each other. The importance of counter-cultures in stimulating personal development becomes obvious. Counter-cultures give students opportunities to confront values and situations they have never faced before, to learn to adapt to them, and in the process to strengthen their own sense of identity. New and different social roles and role models become available to students in cultures which are counter to each other.

Two campus counter-cultures receiving attention now are black power groups and the predominantly white liberal activist groups. These two subcultures appear to share some values in common, e.g. "condemnation of The Establishment." They borrow from each other in terms of life styles, techniques of action, and ideology. Many of the most militant move freely through both subcultures.

Because many of the roles, values, and patterns of adaptation extant in the various counter-cultures are in conflict across the cultural boundaries and especially between counter-cultures and the predominant traditional academic culture, crises develop. Campus crises highlight the differences in values and behaviors among the conflicting cultures, and there are usually dramatic opportunities for student confrontation and, hence, development

(Orrick 70, Cox Commission 31, Kunen 58, Lipset & Wolin 62). Unfortunately, little research has focused on student development in counter-cultures in the campus crisis situation. Counseling psychologists could learn much of value to them and their student clients from doing research on counter-cultures and campus crises.

COUNSELING MINORITY GROUP MEMBERS

White educational institutions, particularly the major ones, are admitting minority group members in greatly increased numbers. Most attention has been given to blacks (Clift 28). These minority group students as a whole present a special challenge and opportunity to counselors in learning to work with them.

Negro students attending interracial colleges and universities are far less likely to drop out before graduation than are white students or Negro students attending segregated colleges (Clark & Plotkin 27).

The dropout rate of the students studied was one-fourth the national rate. Fewer than 10 percent of the Negro students failed to obtain a degree, while approximately 40 percent of white students are college dropouts. The very low dropout rate is hypothesized as due to motivation. The Negro students feel motivated to complete college because to drop out means entering the nonspecialized labor force where their color insures perpetuation of low status. Among dropouts, financial reasons lead all others. Academic success was found to be directly related to parents' occupational level, a common finding in studies of white students. Intelligence tests given in high school were not of much value in predicting college achievement.

Many black students over the last 10 years have moved from a seeking of integration and equality with whites to a search for black power, black independence, black pride, black identity, and for control over their own lives. A number of black students are confused as they seek to cope with their blackness and the expression of it. Thomas (81) discussed the role of the college counseling center in working with black students and challenged counseling centers to give up their social control activities and to be in the vanguard in removing social barriers interfering with the inherent growth force of clients.

Fortunately, there is an increasing amount of literature available to guide counselors in working with minority group students. For example, Washington (91) describes black power and its relevance to the work of the counselor. The May 1970 issue of the *Personnel and Guidance Journal* is entitled "Guidance for Blacks." It is an attempt to acquaint both white and black counselors with the complexity of the situation of black persons, and the various authors make suggestions for working with them.

Vontress (89) suggests in-service and pre-service training designed to help counselors examine their attitudes toward the culturally different and

to expose them directly to the culture of their clients. A book of readings edited by Amos & Grambs (2) provides information of use to the counselor of the disadvantaged.

Hoffnung & Mills (47) found situational group counseling to be effective with disadvantaged youth. Gilliland (41) investigated the outcomes of small group counseling with Negro students. After one year of group counseling, experimental groups showed significant gains in vocabulary, reading, English usage, occupational aspirations, and vocational maturity.

Minority group members on a university campus represent an educational resource for the majority group members. Becoming emotionally as well as intellectually aware of a minority group member as a genuine human being and individual and to learn about his experiences and his heritage is a challenge to the majority person and becomes a part of his general education. In turn, the minority group member can learn from the majority. An important part of general education is learning to tolerate differences among persons and eventually to have an appreciation of these differences through contact, communication, and understanding. White counselors can take the lead in assessing their own attitudes toward minority groups and demonstrating that members of one cultural group can learn to work effectively with members of another cultural group.

CONCLUSION

Counseling psychologists have given most of their attention to helping students with perceived problems who sought counselors' services. If counseling psychology is to assume what the reviewers accept as its responsibility, then counselors must become more creative and active in providing experiences which will stimulate positive development of students who are not focusing on their problems, but who can be persuaded to participate in growth producing experiences. The rapidly expanding literature reporting research on student development should be helpful to the counseling psychologist.

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PERSONNEL TRAINING AND DEVELOPMENT¹

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This review was frustrating for at least two reasons. First, as the term is commonly used, personnel training and development can include such diverse elements as computer assisted instruction, encounter groups for executives and their wives, and training in basic work habits for so-called hard core unemployables. It is an incredibly wide spectrum and certain topics are given scant attention. At the one extreme are very molecular studies of such things as acquisition and transfer problems in pursuit rotor training and concept learning. These border on basic research in learning, more properly belong in such a chapter, and will not be reviewed here. At the other extreme are the existentially derived basic encounter experiences. We will discuss neither the psychology nor the organizational utility of shouting nasty things about one's spouse in front of a group or having a sensitivity session sans clothing.

Second, this is the first review of its kind. Training and development previously have been discussed as a subsection of "the psychology of men at work" (Hinrichs 104) or "personnel management" (Porter 158). Thus, a question arises as to whether the review should cover the past 2 years, 5 years, or hark back to antiquity itself (1940 or thereabouts). As a result, the period of the review varies haphazardly with the particular subtopic. The overall objective was to go back far enough to uncover sufficient substance to describe the state of the art and document trends. In most cases this meant going back 3 to 5 years. Beyond that, secondary sources were employed. Also, since there were no previous chapters to serve as guides, the search for the psychology in the field was especially difficult.

EDITORIAL COMMENTS

By and large, the training and development literature is voluminous, nonempirical, nontheoretical, poorly written, and dull. As noted elsewhere (42) it is fadish to an extreme. The fads center around the introduction of new techniques and follow a characteristic pattern. A new technique appears

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on the horizon and develops a large stable of advocates who first describe its "successful" use in a number of situations. A second wave of advocates busy themselves trying out numerous modifications of the basic technique. A few empirical studies may be carried out to demonstrate that the method "works." Then the inevitable backlash sets in and a few vocal opponents begin to criticize the usefulness of the technique, most often in the absence of data. Such criticism typically has very little effect. What does have an effect is the appearance of another new technique and a repetition of the same cycle. The cycle of fads in human relations training since World War II has been described by Reeves (168).

A fad which is not centered around a technique is the recent discovery of minority and disadvantaged groups. Before 1968 there was almost no mention of disadvantaged minority group training problems. A case in point is the *Training and Development Journal*, which is the principal journal for practitioners. In 1967 there was one article dealing with minority group training. In 1968 there were two, in 1969 there were 15. As a fad, Blacks had caught on. If the fad runs its typical course, there will be a flood of descriptive articles, with a few small-scale empirical investigations mixed in, and the whole business will soon be replaced by some other hot topic. However, in this particular case the consequences are tragic. The problem will not go away.

Another recurring phenomenon is the cyclical article. That is, there are several prototype papers that appear in the literature at regular intervals. For example, there is the article admonishing people to evaluate their training efforts. There is another which argues that training should be well planned and systematic. Yet another reminds us that training and development efforts must have the support of top management or they will fail. There are 5-10 of these basic types, which say the same things over and over again in almost the same language.

The plan of the current chapter is to discuss first some theoretical and conceptual issues that bear on the training and development problem. Second, recent developments in training techniques will be briefly noted. Third, fresh thoughts on the evaluation problem will be discussed. Finally, the empirical literature will be organized around specific topic areas.

MODELS, THEORIES, AND THE TRAINING PROBLEM

Historically, the primary theoretical foundation of training in organizations has been the so-called "learning principles." They are still given a prominent place in the major textbooks (Bass & Vaughan 9, Blum & Naylor 20), and they appear consistently in the literature (Hallstein 91, McCord 139).

Gagne's (77) classic paper pointed out the secondary importance of the principles and suggested that other considerations were far more powerful. He first made the reasonable assumption that total performance can be analyzed into a set of component tasks that are relatively distinct from each

other. Further, proficiency on the task components is what mediates total performance. Given these assumptions, the basic principles of training design consist of: (a) identifying the task components that make up the desired performance; (b) incorporating these component tasks or "mediators" in the training program; and (c) arranging the learning of the components in the optimal sequence for transfer to total performance. This sort of conceptualization implies that training practitioners and researchers should be concerned with such things as task analysis, terminal behaviors, component task achievements, the fidelity of training task components, and sequencing. It places the emphasis where it belongs, on the two questions of what is to be learned and what the substantive content of a training or development experience should be. Once these are answered, the additional questions of what techniques to use, massed versus distributed practice, how to reinforce learner responses, etc., can be considered. Unfortunately, Gagne's statement has stimulated very little activity among people interested in organizational training and development. It stands in a state of suspended animation. What follows is a mere pittance of what there should be.

Considering the question of what is to be learned, an earlier study by Glickman & Vallance (80) analyzed descriptions of specific mistakes made by naval technicians on the job and used this information to alter training content. By using the critical incident method, the question of what is to be learned is answered in terms of actual job behavior and the specification of training content follows directly. This prototype study deserves a great deal of imitation, but there has been very little. In one exception, Folley (71) determined the desired behaviors of retail sales personnel by collecting critical incidents from customers. Over 2000 incidents were collected, categorized, and made the basis for training in customer service. In somewhat the same vein, Buehler (28) developed a series of scaled critical job behavior incidents that defined the "desirability for retention" of clerical personnel. In effect, this was an analysis of total performance into its components.

Backing away from a strict critical incident methodology, Harmon (94) discusses a more molar approach for specifying behaviors to be learned. He presents an analysis of performance objectives for Job Corps curriculum development. Fourteen objectives are grouped under three major headings: verbal performance, physical performance, and attitudinal performance. Each objective is explicated by descriptions of the desired behavior, the conditions under which the behavior should occur, and the success criteria by which the trainee's behavior is to be judged. If a similar plan were followed by the majority of people interested in training and development problems, the knowledge gained would increase exponentially.

An example of the difficulties generated by a retreat from behavioral specification of what is to be learned is illustrated by much of the content of management development. Hersey & Kellner (103) have reviewed many of the "theories" underlying such content. The theory of need achievement

implies that training should create a high achievement need. Theory X versus Theory Y suggests managers should be taught the versatile use of Theory Y. The Herzberg two factor theory says that supervisors should learn to use intrinsic factors to motivate subordinates. Likert's System Four suggests that a participative and consultative approach to management is best. Finally, the original Ohio State variables of consideration and initiating structure appear most forcefully in the Management Grid approach. The prescription is that managers should be taught a style that maximizes both a concern for production and a concern for people.

A controversy has recently arisen over whether the management Grid deserves a third dimension. Reddin (165-167) argues that style theories are not useful because they imply an either/or approach using either the ideal or nonideal style, and they structure the world in terms of a few types instead of several gradations along a continuum. He also proposes that the Grid must be made three dimensional and argues that each combination of employee and production centeredness can vary along an "effectiveness" dimension. That is, there may be situations in which even a 1, 9, 9, 1, or 5, 5, may be effective. The implication is that a manager's use of various styles must take account of situational and individual differences. Hersey & Blanchard (102) sound much the same theme and argue that the proper mix of production centeredness and employee consideration depends in a large degree on the developmental stage in the life cycle of the work group. For example, new work groups may require different management than old work groups. Common (51) also makes the Grid three dimensional by postulating a commitment dimension which implies training should take account of individual differences in the probability of using various styles.

The reviewer feels strongly that the above formulations of what is to be learned suffer because they have no direct link to behavior. The definitions of the variables provide no clear specification of what the desired behaviors are or what the training content should be to insure acquisition of the component tasks. Determining training content thus is a rather large inferential step and depends a great deal upon the artistry of Blake, Likert, McClelland, and the like.

One place we might expect to find a rigorous approach to specifying what is to be learned is in the area of programmed instruction and computer assisted instruction. Not so. Although there is certainly more emphasis on describing terminal behaviors, no recognizable body of methodology for doing so has evolved. As yet we lack even the rudiments of a theory of what is to be learned. Frank & Smith (74) observe that task analysis and behavioral specification of training objectives seems to be a much more prevalent practice in Britain than in the United States.

Attitude Theory

Rather than think of management style as a set of overt behaviors, one could also think of it in attitudinal terms, since it is so often measured via

self report questionnaires. Thus, the things to be learned are a particular set of attitudes, beliefs, or opinions. The attitudinal emphasis can be found in other training activities as well, such as supervisory attitudes toward minority groups. For such reasons, theories of attitudes and attitude change become relevant. Four excellent books on attitude measurement and attitude change have appeared in recent years. Shaw & Wright (184) succinctly compare all the major definitions of attitudes, discuss the theoretical and psychometric issues in attitude measurement, and systematically review almost every published attitude scale in domestic psychology. Fishbein (67) has edited an excellent book of readings dealing with a broad spectrum of topics. Rokeach (172) reviews much of the literature on attitude change, distinguishes attitude from other concepts and presents his own comprehensive theory of attitudes. Kiesler, Collins & Miller (115) spend their entire volume discussing five major types of attitude change theory. Anyone who deals with attitudes as training content should read these four books. All the major issues surrounding how to conceptualize attitudes, how to measure them, and how to change them are discussed. They also document our lack of knowledge concerning the link between attitude change and behavior change. So far none has been demonstrated.

Motivation Theory

Articles on training are fond of saying that the learner should be motivated but things never go much beyond that. The field could profit from a closer look at recent conceptual developments in work motivation.

The work of E. A. Locke and his colleagues (128) suggests a number of important points. For example, the effects of reinforcement on learning may be mediated by a process of goal setting (Locke 126; Locke, Bryan & Kendall 128). That is, unless the learner changes his goals in the training situation, reinforcements such as knowledge of results, money, or recognition may have no effect. Another suggestion is that adopting a specific goal, as opposed to a "do your best" goal, results in greater effort and perhaps better response acquisition. Also, Locke (125) reanalyzed Fryer's data dealing with learning Morse Code and demonstrated that those who said they set high goals acquired correct responses faster than those who set low goals or no goals at all.

Konwin (120) showed much the same thing with female operatives in a stocking factory. The training program was geared to teaching the girls to pace themselves with goal setting. Production went up significantly and turnover went down. However, there was no control group and a placebo might have done as well.

The reviewer has argued elsewhere (42) that some of the effects of goal setting may be due to its task definition component. That is, performance is enhanced because in the process of setting a goal, the individual learns what he is supposed to do, whereas the task previously may have been unclear. Thus, as a training device, performance appraisal by means of mutual goal

setting may be effective because it allows the subordinate to bring order to a previously ambiguous situation. This may be the explanation for the results of a study of T-group training by Kolb, Winter & Berlew (119) which showed that when subjects (graduate students) set personal training goals there were more self and trainer perceived changes. Also, the stronger the commitment to the goal, the more the perceived change. In sum, the data are clear enough to warrant special consideration of how goal setting can be used in training and development programs.

Also, Campbell, Dunnette, Lawler & Weick (42) postulate and cite evidence for the motivational influence of the individual's degree of belief or expectancy that he can accomplish a particular goal or master a particular task. Thus an individual's expectations of how well he will be able to handle the training content may have a lot to do with his motivation in the training situation. The expectancy/valence model is the strongest theme in current theories of work motivation and has considerable potential utility for training and development. Recent elaborations are given by Porter & Lawler (159), Lawler (121), and Locke (127).

Behavior Modification

One of the most important books to be published during the period of this review is a definitive treatment of behavior modification principles and technique by Bandura (7). To date, this model of behavior change has been applied almost exclusively to neurotic, psychotic, or delinquent behaviors. However, the concepts and techniques are quite general, and the reviewer strongly believes it could become a powerful training model in organizations. It is impossible to summarize the wealth of conceptual and empirical information in the Bandura book, but a few points are in order. A briefer but less definitive treatment can be found in Ullmann & Krasner (198). A popularized version is given by Mehrabian (140).

Behavior modification is based in part on the Skinnerian and Pavlovian principles of conditioning, but it goes considerably beyond and takes advantage of many recent findings in basic learning theory. Simply put, it seeks to change individual responses by changing the individual's environment, primarily by altering reinforcement contingencies or stimulus pairings. It is the behavior that is at issue, not the underlying causes or internal mediators. In very general terms, the process would look something like this:

The training objectives must be carefully specified in terms of specific desired behaviors.

For purposes of alteration, the existing reinforcement contingencies or stimulus pairings currently operating must be carefully described.

New contingencies or pairings must be selected that will directly support the desired behavior. The new reinforcers must be powerful enough to alter behavior, and to maintain it once it has been changed.

Change agents must be selected to administer the reinforcement.

If the desired behavior is seldom emitted or is too complex to be reinforced

contiguously, then subgoals must be defined and the appropriate reinforcement contingencies applied to them (i.e. behavior must be "shaped").

After the behavior has been altered provisions must be made for making it self regulating.

Bandura describes six methods of instituting new contingencies: modeling and vicarious processes, positive reinforcement, negative reinforcement, extinction, positive counterconditioning, and aversive counterconditioning. Each is treated at length, examples are given, and the possible uses and limitations of each are discussed. For organizational purposes a combination of modeling and positive reinforcement would seem useful for many training situations. That is, the learner is shown a model who acts in the desired fashion and who is reinforced. The learner is then reinforced for emitting behavior similar to that of the model. Thus many behaviors which previously were not in the individual's repertoire can be generated and reinforced. The book deals with moral and ethical issues at length. In the societal setting, it is both possible and desirable that the change agent and learner agree on the reinforcement contingencies to be used, and Bandura presents evidence to show that an individual's awareness of the contingencies involved has an enhancing rather than distracting effect. A careful reading of the entire book suggests numerous organizational applications. Of course, the major stumbling block is in getting hold of the relevant reinforcers and being able to apply them directly and skillfully to the desired behavior. For example, rather than sending supervisors to human relations seminars to convince them of the desirability of acting certain ways toward subordinates, we should provide them with the correct behavior models and then reinforce the desired behavior with highly valued rewards. Because it would require such a radical reorientation of organizational life, substantial applications of behavior modification principles are not yet possible. However, the mass of data reviewed by Bandura suggests the awesome power of the technique and some tentative beginnings are desirable. Perhaps the employment problems of the so-called "hard core" are a good place to begin.

In a very interesting paper, Nord (152) argues that the operant conditioning model, in truth, has a great deal of structural similarity to the motivational theories of McGregor, Maslow, and Herzberg. It simply gets to the heart of the matter much more quickly.

Individual Differences

The relationship of individual differences to experimental treatments is an honored topic. This review is no exception. At the conceptual level the most important recent publication is a book edited by Gagne (78).

Cronbach (55) conceptualizes the problem of how instruction can be adapted to individual differences. He discusses: adaptation with fixed goals and fixed methods; adaptation by matching the instructional goals to the individual; adaptation by matching instructional methods to the individual; and adaptation by selecting homogeneous groups. He then suggests that two

individual variables of prime importance are general mental ability and the need achievement-fear of failure configuration. Evidence is cited to support his view. Glaser (79) makes a distinction between the traditional kinds of individual differences measures and what he calls initial states, and argues that the latter are potentially more interactive than the former. Thus, variables like prior achievement, training expectations, and initial skill level are more potent than aptitude, interest, or personality measures. Lundberg (131) has outlined some potentially important initial state variables for management development. Jensen (111) specifically rejects the 40 or more individual differences measures developed from factor analytic work and suggests a new taxonomy for explaining the error variance in training results. Perhaps his most important distinction is between intrinsic variables, or individual differences in the learning processes themselves, and extrinsic variables, which have an effect on learning outcomes but bear no resemblance to the learning process itself.

Fleishman (69) summarizes the sizable amount of data he has produced on individual differences in psychomotor learning and makes a distinction between abilities, as relatively enduring traits, and skills, as levels of proficiency of specific tasks. The objective is then to explain task performance skill via the underlying abilities. A taxonomy of psychomotor and motor abilities is presented. A number of studies using different methodologies are used to support the now famous argument that the ability requirements for task performance change considerably over the training period since as learning progresses the nature of the task changes. Also, as the number of trials increases, a larger and larger proportion of total variance is explained by factors specific to the task itself. Fleishman suggests that the ability variables which correlate highly with task performance at various training stages should dictate the instructional cues, or content, emphasized at each stage. Anderson (2) disagrees and argues that ability correlations are not as useful as a rigorous task analysis itself for specifying training content.

Hulin & Alvarez (107) take a more radical approach and argue that an individual's ability level indeed may not be constant and might also change with training. Thus Fleishman's results conceivably could be explained by changes in ability factors as well as task factors. They outline a research study to test the proposition.

Historically, factor analyses of ability tests included in the same matrix with performance scores over trials have also been used to demonstrate the increasing importance of task specific factors during training. Hinrichs (104) conducted a study in which he varied the instructional set in a pursuit rotor task by emphasizing speed in one sample and accuracy in another. Each sample produced two task specific factors, one of which increased in importance over trials and one of which decreased. However, the patterns of factor loadings were somewhat different across samples and conformed to expectations based on the instructional sets. The impact of these results is weakened somewhat by an earlier paper by Humphreys (108) which suggested that

including trial x trial data in a correlation matrix may artifactually yield an increasing task specific factor and a decreasing task specific factor.

General Systems Theory

The "systems approach" definitely qualifies as a buzz word. It is frequently mentioned in the titles of articles (e.g. Miller 142) but typically, few hints are given as to what distinguishes the systems approach from other approaches to training. However, a recent book by Berrien (16) does provide a meaningful explanation of General Systems Theory (GST) that should have considerable heuristic value. Berrien makes some intriguing distinctions between two types of system inputs (maintenance versus signal), two types of system outputs (usable versus waste), and among three concepts of system growth, adaptation, and learning. Simply studying the adaptive mechanisms of the training subsystem (with the heuristics of the GST model) should be an eye opener for most organizations. GST also leads to certain deductions about the relationship of subsystems (e.g. the training function) to the suprasystem (e.g. the organization). For example, a subsystem cannot survive if all its outputs must be directly usable by the suprasystem. The reviewer believes a careful reading of this book will yield a number of useful strategies for the training practitioner and suggest a large set of important hypotheses for the researcher.

To sum up, a more sophisticated analysis of the learning task, a la Gagne, and a more sophisticated translation of work motivation theory, behavior modification principles, and system theory to the training and development enterprise would pay huge dividends in effective behavior change. So far our efforts in these directions have been slight.

NEW DEVELOPMENTS IN TECHNIQUE

Campbell et al (42) document the fantastic amount of training and development activity that takes place in organizations. It is a pervasive enterprise. Almost all reasonably large corporations have formal, in-house training programs, and many corporate training budgets are comparable to the budgets of many large universities. The volume by Campbell et al also discusses 22 distinct training methods that are applicable to management development. The number would be even greater if other types of training were considered. Similar lists can be found in House (106), Blum & Naylor (20), and Lynton & Pareek (134).

There are a number of surveys available (e.g. Foreman 72, Quakenboss 161, and Utgaard & Davis 200) that consider the frequency with which various techniques are used. On-the-job training, job rotation, lectures, and the conference or seminar method are used by almost everyone (i.e. 75-90 percent) while techniques such as simulation, role playing, programmed instruction, and T-groups are used by a relatively small minority (i.e. 15-20 percent). One interesting survey was done by Hannon (93), who asked a sample of 146 firms what they thought of their training and development

programs. The almost unanimous response to this call for introspection was an expressed opinion that things were in bad shape. The reviewer agrees. Following is a summary of the more recent trends in new techniques.

Computer assisted instruction (CAI).—Computer assisted instruction is a sophisticated descendent of programmed instruction and the core elements are much the same. Duncan (63) and Stolurow (193) describe the distinctive features of CAI. A definitive source book is a series of readings edited by Atkinson & Wilson (5). The power of CAI lies in its memory and storage capability which allows many possibilities for interaction (automated) with the learner.

Among many possible interaction modes, the CAI system may permit *drill* and *practice*, i.e. the asking of questions by the student; a *problem solving* mode where algorithms may be called to help the student solve problems; or the *Socratic* mode, in which the computer asks the student questions, the student responds, and the system provides feedback for the response. A more familiar mode is *simulation* and *gaming* which tests the student against some model of the real world.

A very sophisticated form of interaction is the *tutorial* mode. Besides being Socratic in its dialogue, this mode has a managerial component in that different content subroutines and various instructional strategies can be used depending on the initial characteristics of the student and his pattern of responses during learning. Thus it attempts to stimulate much of the complexity of individualized tutorial instruction.

Organization development.—A relatively new approach toward training and development which supposedly takes into account a broader range of system considerations is organizational development (OD). At least the name is new. OD can take one of several colorations. One of the most attention getting is the Management Grid which was recently expanded and codified by Blake & Mouton (17, 18). In its present form it consists of six phases, from the now familiar Grid seminar to a formal critique phase. Unfortunately, the enterprise rests on little data. A second type of OD approach is most commonly labeled "action research." A paper by Beer (12) gives a lucid description, along with a real world example, of what is meant by this term. The process is that the "change agent" must first search the organization for the most fertile ground in which to plant his initial development effort. Concomitantly he must collect data that will tell him if he is on the right track, guide future efforts, and demonstrate the worth of the method to the rest of the organization. If the data are positive, the change effort can be expanded and more data collected. The touchstone is not the content of the change effort but the ever-expanding cycle of action—data, action—data. The process is also described by Zaner (213). A third major thrust is systems four management as described by Likert (124). It is a corporate wide attempt to instill participative and consultive management by means

of conference techniques and the feedback of diagnostic questionnaire results. Finally, laboratory education has been modified increasingly to include groups sampled from a vertical or diagonal slice of the organization (e.g. Benedict et al 14). Thus the interpersonal difficulties, communication problems, and interlevel conflict that hinder organizational functioning can be faced directly.

Basic encounter and personal growth training.—The reviewer senses an increasing emphasis in the T-group literature toward even more existential and less intellectual group experiences. For example, Buchanan & Schindler-Rainman (27) describe the so-called personal growth experience. A parallel development is the use of direct encounter groups with management, as described by Golembiewski & Carrigan (84) and Golembiewski & Blumberg (83).

Television.—Video tape is seeing ever-increasing use as a feedback mechanism. Kennedy (114) suggests how it can be used in training the "hard core," and Stoller (192) describes examples in sensitivity training and group counseling.

Cross-cultural training.—This label does not focus on a new technique but rather on an area in which new methods are needed. The training and development world seems to have discovered cultural differences only recently. Some of the most innovative work has been done for or by the military. For example, R. D. Campbell (43) summarizes the types of programs that have been used to train people for overseas assignments, and Foster (73) outlines a needs and task analysis for cross-cultural training. The non-military literature is more mundane. Lee (122) outlines five problems encountered when developing managers in developing countries but suggests no innovative solutions. Rodgers (171) advocates an on-the-job training and counseling program for developing foreign managers, and Harrison & Hopkins (96) describe a living-in program modeled after the NTL approach. The entire spectrum of cross-cultural studies of organization (not just training) has been reviewed by Roberts (169) and found wanting.

The disadvantaged, the hard core, and the unemployable.—Again, these developments do not deal with a new technique but with a new area in need of useful methods. As was mentioned at the start of this chapter, training and development is a fadish enterprise. Training the disadvantaged became fashionable during 1967 and 1968, and there are now a large number of such programs. A survey by Hausknecht (99) of the Detroit membership of the American Society of Training Directors showed that two-thirds of the member firms were involved with the hard core and 50 percent has a formal training program of some sort. The sources cited below are not exhaustive but are meant to illustrate the major distinctive parameters.

One distinction is between programs run by a community or governmental agency and those conducted by the employing organization itself. Hutson & Smith (109) describe the total community involvement program in Ypsilanti, Michigan. This same program illustrates another distinction, namely, whether or not the trainee is assured of a permanent job if he completes the program successfully. Many government programs financed under the Manpower Development and Training Act, of course, do not.

A major distinction among such programs is in terms of training content. Many of them deal with remedial education (e.g. Murphy 146). Kline (117) describes a computer based system for teaching basic skills. Ten packaged programs are available. Another major content area is specific job training. Jaffee & Friar (110) describe a job training program for secretaries that uses an In-basket simulation. Western Electric uses an entire plant as a vestibule site from which the trainees are assigned to other locations (Chernick & Smith 49). However, most job training is of the usual classroom variety (Doeringer 61). A third major content area deals with attitudes, motivation, and self concept considerations. It is here that most of the innovation has taken place. Ford's Project 250 (Hellervik 101) made extensive use of counselors and role models. The counselors monitored the trainees very closely and were available for assistance at any time of the day or night. Sensitivity training, role playing, and psychodrama have also been used (e.g. Purcell & Webster 160, Hodgson & Brenner 105).

A number of programs have tried to deal with the conflict between the disadvantaged employee and the already established work force. Morrison (145) gave a group of blacks and whites an attitude scale measuring racial attitudes. The participants were required to respond to the questions aloud, and the feelings which were generated became the inputs for group sensitivity sessions. VanBrunt (202) gives an account of using a T-group to overcome the resistance of supervisors to a remedial type training program. Rosen (173) used on-the-job role playing to increase the involvement of supervisors in the special problems of the disadvantaged.

Obviously, specific programs can combine one or more of these content areas. A very comprehensive program which includes almost all of them is described by Adelberg (1).

On the whole, new developments in techniques during the period of the review have not been startling. What has proven noteworthy is the sudden discovery of discrimination and cultural/ethnic differences.

EVALUATION METHODOLOGY

The recurring admonition to "evaluate" training programs is a gross misrepresentation of the empirical question. It strongly implies a dichotomous outcome; to wit, either the program has value or it doesn't. Such a question is simple-minded, unanswerable, and contributes nothing to practical or scientific understanding. The phrase should be banished.

General discussion.—Numerous papers (e.g. Ferguson 65) suggest the use of Kirkpatrick's model for training research. That is, training research should attempt to measure participant *reactions*, or how well they liked the program; *learning*, or the extent to which the training content was assimilated; *behavior*, or the changes in job behavior; and *results*, or the changes in organizational variables such as costs, productivity, and turnover. Catalanello & Kirkpatrick (45) surveyed 110 firms as to which of these variables they used in evaluating their training efforts. Not surprisingly, a high percentage assessed trainee reactions but few tried to measure behavior or results. They conclude that training research is still in its "infancy."

The volume by Campbell et al (42) devotes one chapter to training research methodology. The criteria problem is discussed at length, experimental design problems are reviewed, feedback and systems problems are touched on, and the question of what constitutes interpretable data is faced but not solved. Elsewhere, Campbell (40) has argued that so-called "objective" dependent variables are of little value for training research. What is needed are carefully developed observational and measurement techniques for scaling actual behavior changes. A variant of the method of scaled expectations (Smith & Kendall 187) is suggested as a difficult but very useful strategy.

Dependent variables.—A number of recent studies have investigated new approaches to training criteria. Eddy, Glad & Wilkins (64) let participants develop their own criteria. One thing they found was that there are stylistic differences in individual approaches to training, such as: the traditional desire for knowledge, fear of failing, time filling, and socializing. Baum, Sorenson & Place (10) measured the change in perceived influence of various individuals within the work group resulting from human relations training. An interesting finding was that although the manager's self-perceived influence stayed the same, his subordinates perceived him to have less. Bonjean & Vance (21) attempted to develop a measure of change in self actualization. However, the scales appeared to be nothing more than a means for rating the perceived importance of Herzberg type job factors. In a very interesting T-group study, Dunphy (62) asked undergraduates, over the course of a semester, to write a weekly paper interpreting what was going on in the group. The papers were then content analyzed via a computer program with the aim of measuring role differentiation and describing group phases.

In a pioneering study, Flaughner, Campbell & Pike (68) obtained samples of white and black medical technicians who were rated on a number of job performance factors by both black and white supervisors. There was a slight tendency for white supervisors to rate whites higher than blacks and a somewhat greater tendency for black supervisors to rate blacks higher than whites. It is imperative that we study this aspect of the criterion problem from every possible angle.

Experimental design.—Little is new in the area of pragmatic experimental

design. The volume by Campbell & Stanley (39) remains the definitive treatment. Weiss & Rein (205) challenge the usefulness of the experimental and quasi-experimental approach in complex field research on the grounds that it is difficult to select satisfactory criteria, the situation is essentially uncontrolled, treatments are not standardized, and most experimental designs are too limited in the information they can produce. As alternatives, they suggest process oriented qualitative research, historical research, and case analyses. In a rejoinder, Campbell (38) argues that none of the criticisms leveled by Weiss & Rein are intrinsic to the experimental method.

On a more primitive level, Golembiewski & Carrigan (84) and Becker (11) argue the relative merits of the pre and post measure, no control group design. The former did a study in which they measured the effects of an organizational development program using Likert's 42-item profile of organizational characteristics pre and post. Positive changes were observed and attributed to the OD effort because they were in line with the theory and supported by much anecdotal evidence. Becker argues that such a design may be permissible if the objective is to sell something to management but it is not science.

The statisticians have been reasonably busy arguing about how best to judge the significance of the data yielded by a particular design. Lykken (133) employs a charming, if somewhat anal, example to put yet another nail into the coffin of statistical significance tests and high correlations that make little theoretical sense. However, D. T. Campbell (37) argues eloquently for a restricted but important use for null hypothesis testing.

Fleiss (70) and Vaughan & Corballis (203) derive a number of strength of relationship measures from various design models and discuss their relative advantages and disadvantages. The picture with regard to omega squared is not as simple as it may once have appeared, and these two articles bear careful reading by the research minded. Overall (154) continues previous attempts to take a Bayesian approach to inferring the significance of experimental results. Lord (129) and Werts & Linn (207) discuss the problems concerned with making statistical comparisons between nonrandomized groups, a relatively common problem in training research.

Finally, the time-honored Hawthorne effect has come in for its share of lumps. Sommer (189) makes the general argument that it should not be considered as an experimental error but rather as a much too global label for important independent variables. He suggests we should study the phenomena intensively from a multidimensional point of view and argues that to control for the Hawthorne effect is to rule out much of psychology. For example, if pay effected performance in an unequivocal manner, industrial psychology would become mere economics. Taking aim at the Hawthorne studies proper, Sykes (195) builds a convincing case that economic rewards did indeed play a large part in explaining the Hawthorne data, and Carey (44) argues that the morale or "attention" characterization of the Hawthorne effect was itself confounded with many others in the original studies.

The reviewer agrees that the notion of the Hawthorne effect as a global nuisance variable should be cast aside and replaced with a more sophisticated nonerror oriented view.

Systems considerations.—Lundberg (132) strikes a responsive chord when he argues that regardless of what happens in a management development program the organization may in fact permit no changes in behavior. Thus it would be foolish to expect to find then in a research study. Belasco & Trice (13) and Denova (58) discovered that certain elements in the training program serendipitously increased loyalty and commitment to the organization. In an excellent paper, Goodman (86) outlines the system in which a hard core training program must operate and analyzes possible conflicting objectives. For example, certain conflicts between community agencies and various organizational subsystems are reasonably predictable. Lastly, Greiner (88) presents an interesting descriptive account of the organizational situation in which the well-known Management Grid study of Blake, Mouton, Barnes & Greiner (19) was done. He argues that a number of previously unmentioned situational factors made it possible for the program to have the effects it apparently did, and his description adds a great deal to the interpretation of the results.

A unique contribution.—One of the most provocative methodological papers of recent years is the attack by Argyris (4) on the scientific method as it is used in the social sciences. It should be read and reread by everyone who wants to do empirical research in organizations. The main thrust of his argument is that much of the research in social and organizational psychology creates a Theory X relationship between the researcher and the subjects, with predictable consequences for subject behavior. Research subjects are simply not the passive creatures we believe them to be, and the only way out of the dilemma is to involve them as full participants in the research effort. This rules out such things as deception research, many questionnaire studies, and many studies requiring control groups.

In sum, the methodology of training and development research cries for innovation. Unfortunately, a flight into more sophisticated statistics, even such meritorious ones as variance estimates, does not face up to the principal issue. As yet we have no workable technology that is capable of producing a *large* amount of training research data.

EMPIRICAL RESEARCH

Ideally, this section should be organized around dependent variables. That is, the research literature should tell us what kinds of experiences will produce particular outcomes and what parameters affect these treatment/outcome relationships. Such is not the case. Research studies tend to cluster around demonstrating effects for a particular training technique or focus on specific groups of personnel (e.g. management or the "hard core"). The

subsections that follow represent the reviewer's qualitative cluster analysis of what is published.

Management Development

A base line.—Much of the management development research during the last two decades has been summarized by Campbell et al (42). They review 84 studies and 73 of them are organized via a 3-way classification. One classification dimension is in terms of methods and content. Five categories are used: 1. a broad category labeled general management and supervision programs which are taught via lectures and small group discussions and include material on labor relations, company policies, leadership, etc.; 2. general human relations programs, employing conference methods and role playing, that deal with the human relations problems of supervisors; 3. programs that attempt to teach generalized problem solving and decision making skills; 4. T-groups, sensitivity training, and laboratory education; and 5. specialized programs which are devoted to very specific objectives such as increasing reading speed or reducing the halo error in appraisal systems.

In addition, the studies were classified according to whether internal or external criteria were used and whether the study employed "some" or "few" experimental controls. Internal criteria are outcome measures linked directly to the training content and assessed during or immediately after the learning experience. Attitude measures, achievement tests, in-basket performance, and opinion questionnaires are examples. External criteria are designed to assess behavior changes in the organizational role. By "some" control is meant the inclusion of a control or comparison group. "Few" controls are defined as no control group but both a pre and post measure for the trained group. Given these three means of classification, the 73 studies group themselves as shown in Table 1.

As can be seen from the table, the large majority of studies used internal criterion measures. In over 20 years of research only 21 studies used external criteria of behavior changes. Only 13 of these 21 were what could be loosely described as controlled studies, and 9 of the 13 controlled studies produced statistically significant differences on a major criterion variable. Management development is not exactly a flourishing research area.

Campbell et al (42) generate a number of conclusions from these data.

1. Approximately 80 percent of the 35 studies in the first two content categories produced significant results, but over half of them used one particular kind of criterion measure, namely, an attitude measure of "employee centeredness" or "consideration." Surely this is an overly narrow research view of management development.

In one of the best and most recent studies of general management programs, Schwartz, Stilwell & Scanlan (178, 179) used a time lag design to examine the effects of a university sponsored program on management

TABLE 1. Classification of Management Development Research Studies by Content Area, Type of Criteria, and Degree of Experimental Control

	EXTERNAL Some controls	CRITERIA Few controls	INTERNAL Some controls	CRITERIA Few controls	Total
General Management Programs	2	1	8	5	16
General Human Relations Programs	—	3	10	6	19
Problem Solving and Decision Making	—	1	3	—	4
T-groups and Laboratory Education	6	3	8	9	26
Specialty Programs	5	—	3	—	8
TOTAL	13	8	32	20	73

behavior. The participants were split into two groups which were trained 6 months apart; thus the second group provided a control for the first group to be trained. The Leader Behavior Description Questionnaire (LBDQ) was administered to the subordinates of each group before and after the program. Managers in the first group were described as becoming less active, less definitive, less production centered, less oriented toward supervisors, and more oriented toward subordinates. In contrast, the second group was seen by their subordinates as becoming more active and using more structure. Apparently there was an important interaction between the development program and other parts of the organization such that the second group reacted differently than the first. This study deserves to be emulated. It used a feasible experimental design that provides considerable information.

Unfortunately, almost all of the other studies of general management and human relations training cited by Campbell et al provide considerably less information.

2. Perhaps contrary to expectations, there has been more research on T-group training and laboratory education than any other specific area. T-group studies account for nearly half of those utilizing external criteria. There is evidence that T-groups produce behavior changes in the work role for about 20-25 percent of all participants.

The backbone of the external evidence for the management development utility of laboratory education is found in the perceived change studies, of

which there have been five (22, 30, 141, 199, 201). The primary dependent variable was an open-ended question which asked subordinates and/or associates of the experimental and control subjects, approximately a year after the laboratory experience, to describe specific changes they had observed in the subject's behavior in working with people during the previous year. All studies featured at least one control group. Four of the five chose control subjects by asking the training participants to "nominate" people similar to themselves. Experimental and control subjects in turn nominated several associates as observers.

The results obtained in the Bunker (30) study are typical. Responses to the open-ended questions were content analyzed using 15 inductively derived categories. Eleven of the 15 subcategories yielded statistically significant differences between the experimental and control group with the trained group showing greater change. The greatest differences (20-25 percent) were in areas related to increased openness, receptivity and tolerance of differences, increased skill in interpersonal relationships, and improved understanding of self and others. There were no differences for initiation of action, assertiveness, and self confidence.

While the results of these five studies seem favorable, they must be qualified in view of the way the control subjects and observers were selected. Also, with the exception of Underwood (199), the perceived changes were not evaluated in terms of their influence on job performance.

The T-group studies using internal criteria, which Campbell et al (42) cite, have produced very mixed results. The evidence for laboratory-produced changes in self-awareness, interpersonal sensitivity, personality, and performance on case problems or business games is largely negative. Some positive results were cited for the ability of T-groups to change certain kinds of attitudes, but the data were not overwhelming. In essence, the positive data with external criteria and negative data with internal criteria argue that we do not yet know what aspects of the training experience produce the behavior changes in the organization.

3. Only a very few studies have been made of attempts to teach problem solving and decision making skills and the results have been largely negative.

4. Studies comparing the relative effectiveness of two or more methods for the same goal, or evaluating treatment interactions with individual differences, are too few to warrant making any generalizations. Actually, most of the empirical research deals with just a very few of the many possible methods.

5. In terms of methodological characteristics there is an almost exclusive reliance on statistical significance as a sign of changes to be interpreted. Almost no studies tried to link internal criterion changes with external criterion changes. Finally, no studies attempted to deal specifically with the effect of organizational structure or climate on training activities.

Given the above characteristics of management development research, the following sections summarize some of the more recent literature.

Laboratory methods.—Only two recent studies using an external criterion were found. O'Rourke & Castelli (153) conducted relatively unstructured interviews with the bosses of 27 supervisors 6 months after they had been in a T-group. The results seemed to indicate favorable changes in job performance but the study featured no control group, no before measure, and the bosses knew the reason for the interview. Zand, Steele & Zalkind (212) measured self-perceived changes of manager's job behavior via a 42-item questionnaire. Pre and post measures were employed but no control group. In terms of laboratory education objectives the post measures went down, but returned to the pre training state when the sample was followed up a year later.

In one of the better internal criterion studies, Rubin (175) assessed changes in racial prejudice during a 2-week T-group with a 15-item questionnaire that asked people to rank action alternatives in hypothetical situations. One of two training groups was given a pre-pre measure 2 weeks before the T-group started. Thus the difference between the first and second pre measures constituted the control comparison. The T-group did produce a decrease in racial prejudice and the effect was greater for those low on psychological anomie (measured by a 9-item Guttman scale). There was also a suggestion that the decrease in prejudice resulted from feelings of greater self acceptance. In another good study, Hall & Williams (90) used 20 groups from each of three populations (student, management, and neuropsychiatric patients) to study the effects of laboratory experience on a group decision task—the prediction of juror capitulation in the film *Twelve Angry Men*. Half the groups were experimentals and half the groups were controls and differences were examined on decision accuracy, degree to which the group used its individual resources, and the degree to which the group produced better predictions than any of its individuals. The laboratory appeared to facilitate this kind of decision making, but for some dependent variables, the effect was much greater for ad hoc groups than for established groups.

In four rather poor studies, Slocum (185) demonstrated that college students reported on a pre versus post comparison that they were more sensitive and self aware after a T-group; Finston (66) used an interview to show that good, bad, and so-so reactions to a T-group were distributed about equally in an R & D sample; Golembiewski & Blumberg (82) showed rather weak attitudinal support for the positive effects of a management confrontation experience; and finally, Nadler & Fink (148) demonstrated changes, without a control group, on questionnaire measures of broad democratic attitudes.

Laboratory education is a complex process. Fortunately, an increasing

number of studies are beginning to explore this complexity. Varying the feedback parameter, Myers and his associates (147) asked participants at the end of each session in an eight session T-group to rate each other sociometrically and to predict how others would rate them. They also were given feedback as to how they were rated. The dependent variable was the accuracy of their predictions. One control group made the ratings but was given no feedback and another control group did nothing. The crucial determiner of accuracy was not feedback but whether the participants rated other people. A study by French, Sherwood & Bradford (75) tended to show the same thing, in that "attending" to the variables to be rated produced almost as much change as feedback. When the cue properties of the variables were removed there was a significant drop in rated change.

Culbert (56) varied the amount of trainer self disclosure (i.e. high versus low) and noted its effect on the amount of individual "self-awareness" exhibited in tape recordings of group sessions. There was an interaction between trainer disclosure and the stage of training. To maximize self-awareness, as rated by judges, the trainer should practice high self disclosure at first and change to low self-disclosure later in the training. Cooper (54) studied the effects of trainer attractiveness and the degree to which he exhibited behavior congruent with T-group values. Participants who rated the trainer as attractive and congruent became more like him in both attitudes and behavior (as judged from tapes).

Two studies explored the interaction of the T-group itself with other supporting experiences. Bunker & Knowles (31) reanalyzed the original Bunker (30) data and compared the results of a 2-week versus 3-week laboratory program where the only difference was in the time devoted to lectures, exercises, and theory sessions. Time in the T-group remained constant. Results on the perceived change measure gave a significant advantage to the 3-week program. Friedlander (76) used six questionnaire measures of work group functioning to study the effects of a laboratory program. The content of the laboratory sessions varied considerably, but these differences were completely over-shadowed by the amount of pretraining and post-training follow-up by the consultants. Friedlander also makes some biting comments about the reinforcement contingencies governing consultant behavior and argues that they tend to encourage discrete, short-term training experiences.

Perhaps in reaction to a common criticism, Lubin & Zuckerman (130) used checklist measures of anxiety, depression, and hostility to show that the level of emotional arousal in a T-group is not as great as in a perceptual isolation experiment.

The relationship of individual differences to T-group training continues to be a neglected area. Steele (190) found no relationship between trainer rating of participant change and the *Myers-Briggs Type Indicator*. Harrison & Lubin (97) demonstrated that *task* oriented people were rated as learning

more in a T-group than *people* oriented participants. However, we still do not know which are the more effective group members in the end. In fact, we still know absolutely nothing about how various kinds of people interact with the T-group experience.

Other recent reviews of the T-group literature can be found in Buchanan (26), Delbecq (57) and Mangham & Cooper (136). All of them interpret the data in very favorable, optimistic, and even glowing terms. They see things differently than the present reviewer (e.g. Campbell & Dunnette 41).

Research on management development exclusive of T-groups and laboratory education is abysmally infrequent. Witness the pitifully small number of studies cited below and the meager information they provide. The classification is that used by Campbell et al (42).

General management programs.—Miller (142) used a conference and case method approach to train 54 supervisors in a variety of supervisory topics. As cited previously, a multistage needs analysis was used to group participants in terms of their training needs. Positive results were shown on specially constructed achievement tests. Kohn & Parker (118) report an opinion evaluation by 2000 managers who participated in a 3-day seminar sponsored by the American Management Association. Regression analysis was used to determine the weight of various seminar components in determining overall satisfaction with the program. The opinion components with the largest beta weights dealt with: practicality of subject matter, the balance of participant backgrounds, and the opportunity for participation, all of which is not very surprising. Finally, 6 months after a 1-week development program using lectures and discussion groups, Thorley (197) interviewed the bosses of 234 English managers and discovered a generally favorable response to their subordinates participation in the program. None of these three studies used any experimental controls and taken together they advance the state of knowledge hardly at all.

Human relations programs.—Bartlett (8) asked participants to role play parts antagonistic to their previously determined management style. This led to considerable discussion of the participants' organizational situation, and tapes of the discussion were fed back to top management. There was a subsequent reduction in organizational variables such as costs and absenteeism; however, no controls were provided and the inferences are tenuous. Burke (33) administered scales designed to measure Machiavellianism and social attitudes before and after an undergraduate course in human relations. No changes were observed.

Problem solving and decision making.—There have been almost no recent attempts to study the development of problem solving and decision making skills even though a number of strategies exist for developing these skills. The

closest thing is a study by Moore (144) which compared the business game versus case method in teaching problem solving in production management. There was little difference between the two on an achievement examination.

Training the Disadvantaged

As one might expect, there has been very little published research dealing with training the disadvantaged. As a fad, this kind of training is still in the "let me describe my program" stage, and rewards apparently are given for *having* programs not for sponsoring research. The reviewer suspects, and hopes, that there are many research reports currently residing in corporate and agency files that will soon see the light of day.

Training in basic skills.—Byars & Crane (35) describe a program which was limited to basic skills by default. The New York Public Service Careers Programs sponsored 240 hours of classroom instruction for disadvantaged applicants, mostly female. Significant gains were produced such that most participants were close to passing the high school equivalency examinations, but the job training aspect of the program ran afoul of New York City budget problems. Burcin (32) and Mollenkopf (143) did similar studies and demonstrated significant increases on basic skills achievement tests resulting from remedial training. Patten & Clark (155) did a 6-month follow-up of 48 minority group members who completed a literacy training program in Detroit. The program had made no provision for employment and only 12 of the 48 had jobs. The overwhelming impression from the interview data was the extreme lack of confidence generated by a feeling of being hopelessly undereducated. The study reinforces Rosen, Williams & Foltman's (174) earlier study (and the conventional wisdom) that if a program doesn't lead directly to a job opportunity, morale suffers severely and attitudes toward such training programs become much more negative.

Job skills training.—Two studies by economists attempted to judge the effects of job training programs financed under the Manpower Development and Training Act. Stromsdorfer (194) did a 2-year follow-up of 1379 workers trained under the MDTA in West Virginia. He concluded that earnings and the amount of time employed increased significantly for the trained group. Comparing training costs to taxes paid suggested that the training paid for itself in 13 months. Main (135) surveyed a national sample of 1200 former MDTA trainees and matched them with 1024 untrained controls. After allowing for certain biasing factors, he estimated that the training increased earnings an average of \$10 per week. In similar studies, Solie (188) and Scott (182) also demonstrated favorable results for retraining programs. In spite of their faults, retraining efforts seem to have had beneficial effects.

A massive effort that should be studied intensively is Project 100,000 being conducted by the Armed Services (Wool & Flyer 211). Since 1966, the goal has been to induct an additional 100,000 men per year, primarily by lowering cutting score on the Armed Forces Qualification Test; and as might be expected, people from disadvantaged backgrounds are over-represented. The new entrants are not identified in any way and proceed through normal basic training and advanced training activities. Preliminary results suggest that overall attrition rates and disciplinary problems are much the same as for individuals inducted under the old standards, but performance in advanced skills training is significantly lower. The project represents a golden opportunity to study new training strategies for disadvantaged people on whom we have lots of data. The reviewer hopes it is not lost.

Easily the best and most informative research that occurred during the period of this review is reported by Hodgson & Brenner (105). Two studies in the Lockheed Aircraft Company, one in California, and one in Georgia, are summarized. The bulk of the training in both plants was job related but with some remedial training in reading and shop math. The Georgia program was 12 weeks long, was oriented toward fairly high level training in sheet metal work, included 72 hours of psychodrama and role playing, and the participants were paid only a subsistence stipend provided by a MDTA contract. The California program was only 4 weeks long, was geared toward specific training for specific jobs, and participants were paid a full wage for that job. Considerable individual counseling but no role playing or psychodrama was used. Three-fourths of the Georgia sample were black, two-thirds were 21 or younger, and all were male. The California sample was older and evenly divided among blacks, Mexican-Americans, and whites. It included both males and females. The unique thing about both samples is that the applicants had to meet four of the following five criteria: (a) school dropout, (b) unemployed head of household, (c) income less than \$3000 during last 12 months, (d) poor work history, and (e) no primary work skills. At the California site 70 percent of the applicants were turned down because they weren't "hard core" enough or because their arrest records were overwhelmingly severe. None of the trainees could meet traditional company hiring standards. Dropout rates for the training periods were extremely low. After 4 months on the job, turnover was about 10 percent in both samples, which was significantly below the normal level. A study of the California group showed no difference in quantity or quality of work between trainees and regular hires. The authors attribute the success of the training to appropriate instructional methods, frequent recognition and reinforcement, training content that was directly linked to a specific job, and jobs that were not "dead end."

What have these few studies demonstrated? At least we know that basic skills can be improved and that job training programs can pay off if they are carefully done, include lots of personal attention, and lead to a desirable job.

We know almost nothing about the effectiveness of efforts to deal with role problems arising from a lifetime of discrimination.

Programmed and Computer Assisted Instruction

Programmed instruction (PI).—The research literature on programmed instruction is large. What follows is meant to be a representative sample. Much of the literature is concerned with one-time, one-place comparisons between a PI method and a conventional method using internal criteria as dependent variables. For example, Hedberg, Steffen & Baxter (100) compared a programmed text and a conventional text in teaching insurance fundamentals and demonstrated that the same content could be covered in less time with PI. Subjects also preferred the PI method. Neidt & Meredith (150) also found more favorable attitudes toward PI than conventional texts in an Air Force technical course, and the difference was greater for the high ability students. Mayo (137) summarizes a number of studies on PI conducted in the Navy and concludes that PI yields similar learning in a shorter time than conventional methods, i.e. is more efficient. Comparable achievement in a shorter time seems to be a recurrent finding from PI research. However, there are at least two recent exceptions. Connor (52) compared a teaching machine, PI text, and conventional text for a course in electrical engineering and found that both the teaching machine and PI text produced higher examination scores than the control group. Cavanagh & Jones (46) compared a PI method to three conventional seminar methods for teaching management principles and found considerably higher scores for PI on a multiple choice criterion test.

There are at least two possible explanations for inconsistencies in the above results. First, as shown by Pikas (157), the criterion used to measure achievement may not be "fair" to all methods. For example, Brinkman (25) compared PI to conventional methods for teaching certain aspects of geometry. However, the criterion test was geared directly to the content of the PI mode, which differed from the content of the conventional procedure. Mayo & Longo (138) compared a 9-hour PI course to a 13-hour conventional course for teaching electronics fundamentals and found no difference on a test originally written for the conventional course but a significant difference on a test written for the PI course. Rawls, Perry & Timmons (163) carefully constructed a criterion test that favored neither mode and found equal initial achievement but somewhat better retention and lower variability for the PI group. Long-term retention is largely a neglected research topic. Second, most such comparative studies imply that PI is PI and a conventional method is a conventional method, yet this variability in quality must surely be great within each of these two modes. Comparative studies are difficult because we have no quality model for instructional strategies.

In the organizational setting, external criteria are sometimes used to assess the effects of PI. Patten & Stermer (156) used PI to train foremen in

work standards but the results were equivocal. However, Cook (53) used PI booklets to train regional cosmetic saleswomen and the data suggest a significant increase in sales and lower turnover.

A number of other PI studies have focused on certain parametric considerations. Individual differences, teaching machines versus programmed texts, the amount of material per frame, response mode (e.g. written versus oral), topic arrangement, pacing, and prompting versus confirmation or cueing versus knowledge of results (KR) are the major ones. The latter distinction refers to providing numerous cues or stimulus support before the response is made such that the correct response is always given versus reducing the probability of a correct response and providing post response KR.

Two studies examined prompting versus confirmation. Seidel & Hunter (183) found for a military technical specialty that stimulus support facilitates response acquisition during training but detracts from long term retention. Anderson, Faust & Roderick (3) also showed that heavy prompting hinders post test performance. Variation in response mode (constructive response versus "thinking it") had no significant effects. Berglund (15) varied the degree of partial reinforcement, or confirmation, but found no significant differences on the criterion test. Hartley (98) has reviewed the PI literature on self versus external pacing. The conventional PI wisdom says self pacing is best, but the data are not in complete agreement. A number of studies showed that external controls on the learner's pace had a facilitating effect. Noble (151) and Cahoon, Peterson & Watson (36) investigated the interactions of various measures of individual differences with achievement in PI and found no significant results, which is also a consistent finding in the literature. Johnson (112) generated a number of programs in which the topics were dispersed in varying degrees throughout the program. Although the more dispersed programs required more time, they led to better retention than when all the material on a topic was presented at a single location. Schuster (177) tested a scrambled branching program in which the learner must hunt for the reasons for a wrong answer. He found no achievement differences when the scrambled version was compared to a linear branching version, but subjects much preferred the linear method.

Several studies investigated three parameters at once. Welsh, Antoinetti & Thayer (206) varied response mode, confirmation—no confirmation and the amount of review material in a PI course designed to teach insurance fundamentals. Attitudes and knowledge tests (varying in similarity to the program itself) were the principal criteria. The review material helped slightly, but response mode and confirmation made little difference. Also, PI was only superior to the conventional method on the criterion test most similar to the program. Whitlock (208) varied pacing, number of frames, and KR and found no difference across treatments on a knowledge test. He also makes the cogent point that the better the basic program, the less relevant are parametric differentials. Attwood & Wiener (6, 209) allowed

subjects to select their own level of KR, cueing, and rate of stimulus presentation for PI training in a vigilance monitoring task. KR and stimulus presentation had beneficial effects. Additional reviews of PI studies are given by Schramm (176) and Burris (34).

In sum, the recent PI literature holds no new surprises. A reasonable conclusion is still that, for appropriate material, PI is faster but probably does not lead to greater proficiency on an immediate post test. Long term retention remains an open question. The failure of parametric changes to influence results suggests that the PI enterprise should get back to the basic question of the structure of what is to be learned. The payoff is in the substance, not the form, of the program.

Computer assisted instruction (CAI).—It is apparently too soon in the cycle for a recognizable pattern of empirical studies on CAI to emerge. A great deal of hardware and software is being constructed, but the research efforts are restricted largely to elementary and secondary education settings. Two studies are representative of the small amount of research in organizations. For teaching data processing principles, Schwartz & Haskell (180) compared a PI text to a CAI system that incorporated more branches and skip options. Achievement was the same, but the CAI system was slightly faster. Using a similar CAI system, Schwartz & Long (181) could find few differences between CAI and a self study program for teaching new developments in computer technology. Dick & Lotta (59) compared CAI and PI in a student population and found PI to be slightly superior for teaching numerical concepts. The difference was due to the special difficulties that low ability students had with CAI.

Taken together, these data do not favor the CAI strategy; however, all three studies used CAI systems that were quite unsophisticated and which used little of the potential capability of CAI.

Additional Research Areas

The remaining sections describe areas containing relatively little activity.

Orientation training.—It seems obvious that an individual's initial period at work is extremely critical in terms of how he defines his activities, what he learns to expect from the organization, and how he learns his job. Gomersall & Myers (85) studied a 1-day orientation program for new production employees in a large electronics firm. The training was designed to reduce their anxiety by explaining what the company expected, the hazing they would probably get from experienced employees, and the type of behavior to expect from their supervisor. As compared to a control group, the trained group reached high production levels much more quickly. Lefkowitz (123) experimented with different schedules of vestibule and on-the-job training for sewing machine operators. An initial day in the vestibule followed by a

day on the line followed by more vestibule training seemed to have the most beneficial effects. It is unfortunate that such a fertile area of research has not been tapped to a greater extent.

Sales training.—Another critical area that has not been researched to any great extent is sales training. The recent literature offers only two studies. Harris & Vincent (95) investigated the effects of a 2-year life insurance training program on 88 salesmen who were matched with 88 controls. Significant differences were found on first year premiums and size of policies produced. The investigators ruled out a self selection explanation for the results but could not rule out the possibility of regression effects. Graves (87) also studied life insurance sales training which included a 3-day clinic in role playing interviews. Large differences were found in total sales and the 6-month termination rate.

Training via television.—Although closed circuit television as a fad has run its course, a few studies still appear. Rawls & Rawls (164) compared CCTV to conventional instruction in education psychology and discovered no differences in achievement or retention but very negativistic attitudes toward the TV mode. In addition, students spent almost no time looking at the screen. Rock, Duva & Murray (170) compared CCTV, films, and conventional instruction in a military setting and demonstrated television to be at least as effective as the other modes. Questions of how to improve the quality of TV instruction and overcome possible negative reactions have not been explored. One promising avenue is the use of video recording for providing feedback in certain kinds of skills training. Thoresen (196) used this mode to give history instructors feedback of how they encouraged class participation. Although the data are tentative, the additional feedback seemed to have a positive effect.

Team training.—Training teams as coordinated units should be an active topic, given its potential importance in organizations. Some of the laboratory education and OD techniques to deal with team problems. This empirical research has already been discussed under management development. Blum & Naylor (20) point out that two fundamental questions in team training are the relative effectiveness of group versus individual training of team members, and the kind of feedback that is provided during training.

The Ohio State group has studied two-man teams working in a simulated radar situation where the objective is to track and intercept incoming aircraft. Johnston (113) varied the amount of coordination and communication in the training task and demonstrated that team training was better than individual training when the criterion task required coordination between individuals. Naylor & Briggs (149) investigated the effects of task complexity, task organization, and replacement of team members. They

showed that transfer of team skills varies inversely with task complexity and that task organization was important only under the stress of changing team membership. Strangely enough, the previous training experience of the replacement had only a very temporary effect on team performance. Briggs & Naylor (24) used three-man teams and varied the degree of task organization and the fidelity of the training task (real radar scope versus a cardboard cutout). Team training did not transfer very well when the training simulation was of low fidelity. Briggs & Johnston (23) studied this effect with regard to stimulus fidelity versus response fidelity and showed that stimulus fidelity was by far the most important.

Klaus, Grant & Glaser (116) used another laboratory task to examine the effects of different reinforcement patterns on team training. A clear finding was that team or group reinforcement must be supplemented by individual reinforcement both during training and in the actual performance situation. Team reinforcement alone is not enough.

The above studies suggest that team training has value for transfer to tasks requiring interaction, but the additional increment is quite fragile and can be largely obviated by such things as lack of stimulus fidelity in the task or lack of individual reinforcement. However, the tasks used to study the benefits of team training have been quite limited and we have no good definitions of such concepts as team, coordination, or fidelity.

Simulation.—Again, the reviewer anticipated a large amount of material and again there was not very much. Two studies dealt with fidelity problems in simulated training equipment. Hammerton & Tickner (92) varied the fidelity of the remote operation of a toy trolley. Background detail and depth cues during training were crucial for initial performance but training fidelity had few effects on long term retention and performance. Essentially the same finding was demonstrated in a study by Grimsley (89) in which service personnel were trained on guided missile system controls of varying fidelity. Few differences in performance were observed when the criterion data were obtained 4 to 6 weeks after training.

In management development there exist a large number of simulation methods such as business games, case problems, and role playing. However, these seldom find their way into research studies such that their unique effects can be studied (Campbell et al 42). They are most often combined with other techniques and the overall package is "evaluated." The business game is an infrequent exception. Raia (162) demonstrated that playing a business game improved test performance in a business policy course. Two levels of game complexity (fidelity?) did not influence the results. Steinmetz & Patten (191) showed only that students reacted favorably to using a business game in an accounting course.

Specialty programs.—These studies are grouped here because they deserve

mention but do not fit anywhere else. Chaney & Teel (47) report an excellent study of improving quality inspector performance by means of classroom instruction plus visual aids. A control group was compared to three experimental groups: classroom alone, visual aids alone, and classroom combined with visual aids. The training content was specified by a careful analysis of inspector performance, and the criterion was the percent of defects subsequently discovered on the job. The largest increment in the criterion resulted from using visual aids.

In one of the few cross-cultural studies, Chemers (48) used the "culture assimilator" to train Americans in Iran. The technique asked the participant to analyze hypothetical situations in a specific culture and then provided feedback for right or wrong analyses. When asked to lead three-man Iranian problem solving groups, the trained Americans were found to generate more favorable group climates and better leader-member relations. However, group productivity did not change.

Finally, Viteles (204) reports a long range follow-up of executives who participated in the University of Pennsylvania's humanistic studies program. The trends toward more liberal attitudes and less emphasis on economic values which were found in the original study tended to hold up over the period of the follow-up.

Summary of Empirical Results

What do we know? We know that management development can change managers toward more employee centered attitudes. We know that laboratory education probably can change behavior in the work role, but the nature and implications of these changes are unclear. We know that PI is at least as effective as conventional instructional methods in some situations and is probably faster when used appropriately; however, many parametric and situational contingencies remain unspecified. We know almost nothing about what makes orientation, sales, or team training effective. Hard core training programs can produce valuable outcomes but we really don't know why. We know that technical training, as well as remedial training in basic skills, does produce significant increments in knowledge. We do not know anything about the relative effectiveness of different training strategies for achieving particular objectives. In sum, we know a few things but not very much.

CONCLUDING REMARKS

Even though the study of training and development problems has produced a certain amount of knowledge and there are grounds for a bit of optimism, one cannot come away from this literature without feeling disheartened. The yield of information is depressingly small, and if current trends are extrapolated, there are few reasons for expecting any significant increase in the rate of return. We simply must get off the trend lines we are currently following. Potentially, the training and development enterprise is

an awesome instrument for change. However, there are both internal and external reasons for why it has not realized its potential.

The major internal reason is that there has been a broad and full scale retreat from the fundamental task of defining what is to be learned. If this review illustrates nothing else, it indicates that in area after area researchers and practioners have retreated from a concern with the structure of what is to be learned to an almost exclusive concern with hardware and techniques. This is as true for computer assisted instruction as it is for laboratory education.

The major external reason is that rewards are given for putting together programs, courses, or automated systems that are attractive, and that elicit a favorable impression from trainers, trainees, and sponsors. There are few incentives available in public or private institutions for attracting the personnel and stimulating the kind of effort it will take to develop sound theoretical models, build a technology of assessing what is to be learned, and accumulate a broad data base. As a nation, we seem unwilling to devote many resources to these kinds of things even though many of our most crucial national problems stem from an inability to educate, train, and develop.

These phenomena are nowhere more apparent than in our approach to the so-called hard core. Not too long ago organizations were finally coerced into discovering that unequal opportunity created problems. The training response has been to proliferate relatively short term "programs." At best this response had had a very limited success. At worst, it is mere tokenism. The training establishment has failed to describe hard core training needs in specific behavioral detail and to directly link training content to desired behaviors. But then why should it? It is not reinforced for doing so. Our collective tragedy is that this potentially lethal problem will become larger and larger and no long term solution will result. We must become more political as well as more empirical and theory oriented.

In spite of these heavy constraints, the few bright spots in this review suggest that some progress can be made. If we are ever to make training and development a profitable enterprise in terms of important behavior changes we must at least do the following:

1. Devote considerable time to an empirical analysis, via the systems approach, of the training and development system and its interactions with other systems. This must go considerably beyond putting general labels on block diagrams.

2. Take an intelligent plunge into the methods and concepts of behavior modification. It holds considerable promise.

3. Adopt the PI model for *every* training activity. That is, our task should be to specify terminal behaviors, decompose the learning task into its structural components, and seek an optimal sequencing of these components. This is rational not mechanistic behavior.

4. Forget about the either/or approach to training evaluation. Instead, we should worry about measuring behavioral outcomes and their interaction with other subsystems in the organization. We should also worry about the differential effects of competing training strategies. Knowing these differential effects is the ultimate payoff.

In moments of guarded optimism, the reviewer believes that moving in these directions would provide substantially greater knowledge and far more beneficial practices than have characterized the field during the last 20 years.

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